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2

THE

SAINT LOUIS

Medical & Surgical Journal

BI-MONTHLY.

NEW SERIES. 1866. VOL. III.

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"Liberam profitetor medicinam, nec ab antiquis sum, nec a novis; utrosque ubi veritatem colunt, sequor."—KLENIUS.

ST. LOUIS, MO.

1866.

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THE SAINT LOUIS

Medical and Surgical Journal.

JANUARY AND FEBRUARY, 1866.

Original Communications.

BIOGRAPHICAL SKETCH OF CHARLES A. POPE, M. D.

Dr. Pope was born in Huntsville, Alabama, on March 15, 1818, consequently, being now in the 48th year of his age.

His primary education was received at Greene Academy, in his native town. He subsequently passed through the prescribed courses of collegiate study in the University of Alabama, and immediately afterwards commenced the study of law. Finding, however, that the sedentary habits necessary to its prosecution were incompatable with his then rather delicate constitution, he abandoned it for the study of medicine.

He began his medical studies with Drs. Fearn and Erskine, of Huntsville; the former being already widely and well known as a surgeon, and the latter ranking high as a physician. For both these gentlemen, Dr. Pope has always entertained high admiration and regard; he has always looked upon them as perfect types and

exemplars, in a social as well as a professional point of view. It was doubtless fortunate for the subject of this notice, that his early professional education was confided to such men. His own early eminence may be in a great degree attributable to their influence and example.

Dr. Pope attended his first course of medical lectures in the Cincinnati Medical College. Dr. Drake was then at its head, and in the height of his fame and popularity as a teacher. He was the idol of the class, and has always been regarded by Dr. Pope as one of the "greater lights" of the profession in this or any other country or age.

At the age of 21, Dr. Pope received his degree from the University of Pennsylvania, and soon after repaired to Europe for the further prosecution of his studies. It was in the city of Paris, in Nov., 1839, that the writer of this brief notice first became acquainted with the young surgeon, for he had already begun to manifest his predilection for this branch of the profession. He was then noted as the most expert and neatest operator on the cadaver, amongst us. His friends, acquaintances, and teachers, even at that early day, predicted that he would in after years rank with the renowned masters of the noble and daring art.

He remained in Paris nearly two years, and afterwards visited the great continental schools, as well as those of Great Britain and Ireland.

It was on January 1, 1842, that Dr. Pope commenced the practice of his profession in the city of St. Louis, where his studious habits, high moral worth, and gentlemanly bearing, soon brought him into public notice and favor. In 1843, he was unanimously chosen Professor of Anatomy and Physiology, these chairs being then united in the Medical Department of the St. Louis University, a position for which he was not slow in proving himself fully competent. In 1846, he again visited the Old World, and furnished, during his absence, various contributions to the *St. Louis Medical and Surgical Journal*. In 1847, he was transferred to the chair of Surgery, at which time he had acquired

considerable reputation as a judicious surgeon and skillful operator. It cannot but be regarded as a fortunate circumstance for him, that he delivered three courses of lectures on anatomy, before his accession to the chair of Surgery; to this fact, no doubt, is to be attributed in a great degree his rapid success as a teacher and practitioner of this his favorite branch. In 1848, he was chosen Dean of the Faculty, an office which he has ever since held.

As a lecturer, Dr. Pope is rapid, clear, and concise; he is fluent, but makes no effort at oratorical effect. Few men can say more, or teach more, in an hour, than he. With the history and literature of surgery he is perfectly conversant. He is familiar with all the doctrines and modes of operating extant, and well may he be, as his entire attention is, and has been for years, devoted to them. He eschews the practice of medicine, consecrating himself solely to the teaching and practice of surgery. His collection of instruments, preparations, drawings and models, illustrative of his branch, are unexcelled by any private collection in the country.

As an operator, he is calm, confident, expeditions, full of resources, and, it may be added, eminently successful. Perhaps no surgeon of his age, in the country, has performed as many operations as he. This fact is not attributable to the absence of competition, for he found, on his arrival in St. Louis, three surgeons high in public estimation. Dr. Beaumont was then in his glory, and he, venerable man, was among the first to discover and rightly appreciate the talents of the future surgeon of the far West. Dr. Pope's connection with the St. Louis Hospital, as well as with the City Hospital, and the circumstance of St. louis being a centre, attracting surgical cases from the surrounding States and Territories, will in part, perhaps, account for his extensive practice and frequent operations. His contributions to surgery are valuable, and have been extensively copied and frequently referred to as of standard authority.

A proper notice, however brief, of Dr. Pope's career thus far, requires a tribute to his liberality and devotedness to the Medical

Department of the St. Louis University. To him, in a great degree, the school is indebted for its success; and, in this connection, justice demands the mention of the name of his father-in-law, Col. John O'Fallon, a man whose princely wealth has always been employed in behalf of the best interests of his city and his State—of humanity and of science. To him, the subject of this memoir and his colleagues are indebted for the ample halls of the St. Louis Medical College, and the public Dispensary thereunto attached.

Ten years ago, one of Dr. Pope's colleagues spoke of him in a public address, a valedictory, as follows: "Our professor of anatomy is yet a very young man, but I predict that in ten years he will rank among the first surgeons in the country. That prediction has been fulfilled, and Dr. Pope is yet a young man. Ten years ago, St Louis was not what it now is; then it had only a population of 40,000 souls, now it contains 130,000." [The ten years, with ten more, have passed, and the population is now 210,000.] What will the city and its institutions be in ten or twenty years more? May they not fully rival those of the East and of the Old World. May not the name of the young surgeon of the west be placed with the most venerable names of the profession? and may he not bequeath to the institution which he has so efficiently contributed to build up, and to the profession, a fame equal to that of a Wistar or a Physick?

At the Annual Assembling of the American Medical Association, at St. Louis, May 2nd, 1854, he was unanimously chosen President, and presided over the meeting with distinguished ability. Since that time Dr. Pope has been almost unremittingly engaged as lecturer upon and practitioner of surgery in St. Louis. The number of cases in which he has operated for stone, is about on a par with his surgical cases in general. He has operated for stone in the bladder seventy-seven times, the number of successful operations being seventy-three, only four deaths having occurred in his practice. This is certainly a very fair surgical record.

In 1864 Dr. Pope again visited Europe, where he spent nearly a year, most of the time amid the scenes of his early studies.

He was much with Velpeau, whom he so much admired a quarter of a century ago, and pursued his favorite study as though he were commencing it anew. In the present session he again fills his chair of Surgery in the St. Louis Medical College. Previous to his departure for Europe, he was tendered a public dinner by the medical profession of St. Louis. This was a splendid ovation, an account of which was published in the *Journal* and daily papers of the time. The speeches elicited at that reunion would, of themselves, constitute a history of his brilliant career. We reproduce only a poem, recited on the occasion by a senior member of the College to which he belongs:

A REMINISCENCE.

"Far, far away, long years ago,
I was beyond the sea;
One morning a slim stripling called,
And reached his hand to me.

"I had a cold, was muffled up,
And lean as any lath;
At once I held out both my hands—
My feet were in a bath.

"I liked his looks, he seemed to me
A very clever chap;
I even liked the way he wore
His blue-cloth student's cap.

"So much I liked it that, although
I had not much of pelf,
I went out in a day or two,
And bought a cap myself.

"I walked with him the hospitals,
And listened with him to
Andral, Velpeau, and Marjolin,
Rostin, Dubois and Roux.

"I saw him in dissecting rooms,
And fancied, in a trice,
That he would a great surgeon be,
He cut up things so nice.

"The winter past, spring came and went.
I knew he'd be a man,
And so I talked with him about
His hopes and future plans.

"Said I, 'Young man, of course you'll act
According to your will,
But I advise you settle down
In good old Louisville.'

"I thought to live in Louisville
The very height of luck;
Such were my ideas, long ago,
In dear old, brave Kentuck.

"Some two years passed, I pulled up stakes,
And found myself right here;
And who but this same slender youth,
Should all at once appear!

"I knew him well,—was very glad
Such pleasant sight to see;
I always said to every one,
He would a surgeon be.

"He came with me into the school,
It was in '44—
Stop, let me think—no, I mistake—
It was the year before.

"I made a speech about that time,
In which I shrewdly guessed
That ten years would enroll him with
The surgeons of the West.

"Those years passed by, and he had done
More than I prophesied;
And so I said, ten years ago,
'Let ten more years be tried,

"And you will stand amongst the first
Upon a hemisphere.'
The time is past, the deed achieved—
The conqueror is here!

"Retire, my friend, and rest awhile,
And then return again;
Gird on your armor, brace your nerves,
And try another ten."

Dr. Pope has again girded on his armor and engaged in the other "ten"; and as he is yet but in the meridian of his usefulness, we may reasonably hope that he will not only accomplish them, but live many years afterwards,—an ornament to his profession, and a benefactor of mankind.

FIBROUS TUMOR OF THE WOMB, OPERATED UPON THREE TIMES WITH THE ECRASSEUR, AND FINALLY DESTROYED BY PRESSURE.

BY THOS. KENNARD, M. D., OF ST. LOUIS.

On the 24th of August, 1864, I was, through the recommendation of my friend, Dr. Boisliniére, summoned to see Mrs. H., a married lady about 36 years old, mother of one grown daughter, by her first husband. By her second husband she had one child, which died soon after birth, and two miscarriages, the last when about three months pregnant. For six weeks previous to my first visit she had been attended daily by two quacks, one male and the other female, who had been treating her all this time for retention of urine, administering balsam copaiva, spts. of nitre, and other diuretics, *besides* introducing the catheter, to relieve her bladder, once or twice daily. She had suffered greatly from uterine hemorrhage, was very anæmic from loss of blood, and much emaciated by anorexia and confinement to bed. Up to a short time previous to the commencement of her retention of urine, she had not been annoyed by anything except irregularity of and excessive menstrual discharge, which frequently amounted to violent and dangerous hemorrhage, for which various injections had been employed without benefit. I found her exceedingly weak from loss of blood, but complaining only of her urinary difficulty, and mentioning the hemorrhage only after being closely interrogated by me. On making a vaginal examination, I was very much surprised to find a large and very peculiar tumor, distending the entire vagina very much, pushing up the womb beyond the reach of the finger, and pressing so much upon the urethra as to produce complete retention of urine. I relieved the bladder by means of the catheter, and on making a very careful examination of the tumor, and listening to her account of the first attack of hemorrhage, following immediately upon a miscarriage, coming on suddenly upon rising from bed. I was pretty well con-

vinced that I had a case of inversio-uteri to deal with, supposing that the womb had become much enlarged in its unnatural position and condition. The tumor had every appearance and feel of an inverted uterus, as described by the best authorities upon that subject, except that it seemed more incompressible than it should have been. Several of our most distinguished obstetricians and practicing physicians, who saw the case, expressed themselves, on first examination, as being convinced that it must be inversion of the uterus. However, failing in my repeated efforts to return it, and remembering that if the uterus was really, as we supposed, inverted, the vaginal canal must of necessity be very much shortened, instead of being lengthened, as it was. I took a long silver male catheter, straightened it, and passed it up the vagina as far as I could without using violence, and found that it passed about thirteen inches anteriorly and nine posteriorly,—passing, no doubt, in from along the side of the tumor into the cavity of the womb, and thus firmly convincing me that it could not by any possibility be an inverted uterus. These facts also convinced the other gentlemen that the tumor was not, as at first supposed, an inverted womb. What, then, could it be? To the finger the tumor felt like a distended bag with *very thick* walls. It was not hard and irregular, as we generally find fibrous tumors, nor had it the feel of any kind of polypus, except what is described as the *hollow polypus*; and the question was, could it be this? It certainly resembled it very much, but on a very careful examination of the abdominial cavity, we detected another small tumor, evidently attached to the external surface of the womb, and having all the feeling of an ordinary fibrous tumor. This fact, connected with the hard sensation of the tumor high up, just as it entered the mouth of the womb, and its evident firm attachment to the os uteri posteriorly, convinced me that it must be a fibrous tumor. Accordingly, Drs. Boisliniere, Pallen and Prewitt, concurring with me in opinion as to its nature, I concluded that it had best be removed by the ecrasseur, at least as much as we could reach and encircle. On September 15th, at my request, Professor Pope operated for its removal. After getting the pa-

tient completely under the influence of chloroform, and placing her in the usual position for lithotomy, we seized the tumor with two large volcella or toothed forceps, and attempted to draw it down through the vulva, so as to pass the chain of the ecrasseur around as much of it as possible. It yielded very little, however, to the greatest amount of force admissible, and after many endeavors to push the chain up near to the mouth of the womb, we failed, and Dr. Pope had to content himself by cutting very slowly through about one third of the tumor, which, as it became compressed by the chain, was very tough, and required great care to avoid breaking the instrument. But slight hemorrhage followed the operation: the patient was placed comfortably in bed, and some lumps of ice occasionally passed into the vagina, during the evening, to prevent any flooding, and the patient watched closely for a few days. No fever nor unpleasant symptoms followed, and in two weeks the patient was able to be about and attend to her household duties. No excessive discharge of blood at the menstrual periods took place after this, nor did any more hemorrhage occur. The pressure from the urethra being removed, of course there was no longer retention of urine. I gave the patient tonics, and the solution of per-chloride of iron, internally and by injection, as an astringent and styptic. Three or four weeks after the operation I found the cut surface rounded off, and the tumor beginning to grow again. It increased so rapidly that by October 29th a second operation was necessary to relieve retention of urine and the irritation of the vaginal walls. Dr. Bernays, assisted by Drs. Prewitt, Lingenfelder and myself, after prolonged efforts, succeeded in removing considerably more of the tumor than could be gotten away at the first operation, but did not excise one half of the tumor. Considerable hemorrhage followed this operation: the tumor seemed to have become much more vascular. In the course of forty-eight hours, symptoms of peritonites manifested themselves, and for more than two weeks the patient was so ill that I despaired of her life. She, however, finally recovered, and was again able to walk about and manage her household. From this time until May, 1865, she got along

recovery well, but the tumor grew rapidly, and became much larger than ever,—completely filling up and projecting from the vagina, causing retention of urine, intense pain, and impossibility of movement. On June 20th, 1865, a third operation being indispensable, Professor Hammer, assisted by myself and others, again used the ecrasseur, removing about as much as at the second operation. Considerable hemorrhage ensued, the tumor having very much changed since the second operation, both in firmness and vascularity—there was none of the sack-like feeling so manifest before the first operation was performed. The patient seemed less affected by this than either of the previous operations, as I did not have to make her but three visits before she was able to be out of bed again. I only visited the patient occasionally after this, until the first of September, when the tumor had again grown larger than ever, and by its pressure had produced great œdema of the lower extremities, excessive irritation of the vagina, and great difficulty in walking. The patient soon became bed-ridden, and suffered great agony from neuralgic pains. Having now operated three times, with the effect of giving temporary relief, but each time increasing the rapidity of growth and size of the tumor, I despaired of doing any permanent good that way, as each operation had failed to so destroy the vitality of the tumor as to cause sloughing, which I had hoped would be the case. I concluded to try if constant pressure by a firm support might not effect the purpose which the operations had failed to do,—or, if not, at any rate to mitigate the suffering of the patient by removing the pressure from the urethra and vulva. For this purpose I procured a cork pessary, coated thickly with wax, and attached to a strong wire stem. Placing the patient upon her back, with her hips elevated, I pushed the tumor up as far as possible, then introduced the pessary, placing it firmly against its lower surface, and retaining it in situ by means of straps buttoned to a patent elastic belt or abdominial supporter. By this means I at once succeeded in relieving the pressure very much, and rendering walking quite practicable, and to her comparatively comfortable. She wore this with great relief, until about the last of the month.

The dropsy was relieved, and the patient enabled to remain much of her time on her feet. The irritation from the tumor now became very great, and a profuse offensive discharge commenced, accompanied by continued irritative fever. After a few days' confinement, and continuance of the low fever and fœtid discharge, I became satisfied that the vitality of the tumor had been destroyed by the constant pressure of the pessary, and sloughing had actually commenced. About the 10th of October a considerable portion of the tumor sloughed, and from that time until the 20th, portions continued to come away, accompanied by a profuse and excessively fœtid and ichorous discharge. The low fever continued, and in spite of stimulants, iron, quinine, whiskey and beef tea, given as freely as demanded, the patient continued extremely low, and I supposed that every day would be her last. On the morning of the 20th, her ever faithful nurse told me that the tumor had come down considerably during the night, and on examination I found a large putrid mass protruding through the vulva. I gave a full dose of whiskey, wrapped a cloth around the mass, and with gentle but continued traction, succeeded in removing the whole of the remaining tumor, a portion of which was still adherent to the inner surface of the uterus. The patient did not suffer very much, and only slight hemorrhage ensued; and although now excessively low, she was relieved of all pain, and did not seem much weaker than before its removal. A half hour afterwards she had fainting spells, and in spite of stimulants and absolute quiet, it seemed impossible for her to rally. However, after a few days she began to gain a little, and has continued improving, until now, thirty days after the last operation, she has entirely recovered—at least is out of danger,—has been sitting up for twenty days past, is improving rapidly in health, and, so far as her uterine trouble is concerned, may in future enjoy good health.

This, to me, was a very remarkable and interesting case, not only from its obscure history; its resisting three surgical operations, at each of which a large portion of the tumor was removed; its large size, weighing in all *about* seven pounds, and its pecu-

liar and favorable termination, but also the great tenacity of life manifested by the patient, and the modus operandi of its cure. I think there can be no doubt in any intelligent medical mind that the pressure of the pessary upon the tumor obstructed its circulation, and finally destroyed its vitality, and thus produced a sloughing. This was suggested by Dr. Hammer, in some remarks upon the subject, before the St. Louis Medical Society, and was one motive which first induced me to use the pessary. It may be suggestive as a means of cure in other tumors which cannot be better treated. The tumor was not pedunculated, but attached irregularly by about one-fifth of its surface, and must have been at first interstitial. It was utterly impracticable to apply a ligature so as to remove it.

AN INTERESTING CASE OF NECROSIS OF THE FRONTAL AND PARIETAL BONES.

BY J. M. YOUNGBLOOD, M. D., ST. LOUIS, MO.

In November, 1861, I visited a lady, in company with Dr. Kirkpatrick, from whom I obtained the following:

Mrs. ——, aged about 35, the mother of two children, both very healthy. She was attacked, about three years before my visit, with a very severe toothache in one of the upper molar teeth on the right side. The tooth was extracted without relief to the patient, the pain extending to the right eye. Local appliances and medicines internally administered, gave but the slightest temporary relief. The disease in the eye continued to the destruction of the entire ball, without cessation of pain, and continued its upward course, through the front and upper part of the head. After many months of the most excruciating pain in the head, the

scalp began to slough, and soon left a considerable portion of the os frontis exposed. Not long after this the patient discovered that the bone itself was loosened, and after picking at it from time to time, it dropped slightly forward, and lodged with its lamer and anterior border on the bridge of the nose. Some time afterwards the patient lifted the bone away. The dura mater was left entire, with all that portion lining the os frontis, and a part of the parietal and temporal bones, exposed. In addition to this loss of bone, there was a portion, circular in form, that gave way in the prominent part of the right parietal. This was about the size of a silver half dollar, having an opening similar to that made by a large sized trephine. The margin of bone in the lesser, as well as that in the larger opening, was rough and jagged, as if broken by direct force. After the removal of the pieces of bones, the patient was relieved of pain, her appetite was regained, and her general condition rapidly improved.

On making my visit, which was nearly twelve months subsequent to the above, I found the patient seated by a fire, knitting. She was working briskly, her cheeks were tolerably full and round, and of a good color. On inquiry she informed me that she was in better health than she had been for the last three years. I requested her to remove the cloth from her head, as I desired to determine the extent of the disease. On the removal of the cloth, and the strictest examination I could make with the surrounding circumstances, I found that nearly the entire os frontis was removed, with a considerable portion of the parietal bones ; for the line of demarkation between the diseased and the healthy portions of the bone, was not drawn at the coronal suture, but through the very substance of the parietal. In addition to this, there was the circular portion of bone, previously named, from the substance of the right parietal. The dura mater was entire, and in a healthy condition. It was stretched tightly across, and remained firmly attached to the bones just below their broken margin. The scalp was contracted upon itself, leaving its margin thicker than the corresponding portion, in health. I could raise it and trace the rough margin of bone in its entire circle. From the border of

the scalp around this opening, there issued a small quantity of healthy pus. Some of the blood vessels of the dura mater were very perceptible, and could be traced by the eye over a considerable portion of the exposed surface. The color of the dura mater was whitish, and there appeared to be no indications of inflammation. I could and did press considerably upon the membrane, without observing any marked symptoms. The hair on the remaining portion of the scalp was in a healthy condition, and she kept it neatly tucked up with a comb. At the point of the dura mater corresponding with the anterior fontanel, was a regular and marked pulsation. All she complained of was a dull, heavy headache, and a partial blindness whenever that part of the head was exposed to an elevated temperature. She ate heartily, rested well at night, could walk about, and do the ordinary work she had heretofore been in the habit of doing. There was no perceptible deficiency in the functions of the brain, notwithstanding a very considerable portion of it had, probably, been removed; for, instead of that rotundity of the forehead, there was a considerable excavation.

Dr. K. had been acquainted with the patient for several years, and stated that he could discover no deficiency in the mind. During her illness there was no marked symptom about the head, except that of intense pain. She had no pain in any other part of the system. All other bones of the body seemed to be in perfect health. When I saw her she was free from pain, and said she enjoyed very good health. Her appearance fully convinced me that, as far as the assertion applied to her general system, it was correct. On the 4th of July preceding my visit, she walked four miles, besides remaining on her feet the greater part of the day, and officiating in various little things peculiar to the occasion.

The Doctor informed me that she had been frequently profusely salivated: that her husband had been in the habit of giving her calomel on the slightest appearance of disease, even for a bad cold, and in this way had caused ptyalism at different times. By this means she had lost some of her teeth, the others remaining only moderately tight in the alveole. I secured the specimens of

bone, but unfortunately do not have them with me. I hope to receive them before a great while, when I will take great pleasure in exhibiting them to the Society. I would have reported the case previously, but for the fact that I had not received the specimens, otherwise I would have been enabled, as I very much desired, to have exhibited them to the Society with the report. The exact dates I have not been able to give, as I have written entirely from recollection of the case.

GUN-SHOT FRACTURES OF THE SKULL.

BY S. B. HOUTS, M. D., ST. LOUIS, MO.

Gun-shot fractures of the skull have, at all times, been regarded as dangerous in character, and most fatal in their final results. This, I believe, has been the experience of surgeons in the late war, on both the side of the U. S. A. and C. S. A. The treatment of these accidents have been somewhat different, some preferring what is termed the more *radical* procedure, *the immediate interference of art.* Other surgeons, taking a conservative course, merely assisting the efforts of nature in the process of reparation. In furtherance of the latter method of treatment, I wish to add a little of my experience, by giving the details of several cases of *gun-shot fracture* of the skull.

CASE 1. Gilbert A., belonging to the 12th Iowa Regt., at the battle of Shiloh, was hit, by a piece of shell, over the prominence of the left *parietal bone*, crushing the skull at that point to the extent of the circumference of a half dollar. The soldier was captured, marched to Corinth, Miss., a distance of twenty miles; from there transported in freight cars, with many other prisoners, many of them wounded, to Mobile, Ala.; thence to Montgomery,

Ala., by steamboat, arriving there April 15th, making nine days from date of reception of wound; during which time there was no opportunity for extending surgical treatment to the wounded. Their condition can easily be imagined, after being conveyed thus, far through a damp, malarious atmosphere, chilly rains, clothes stiffened with dried blood, pieces of woolen fabric hanging from the wounds, leaden missiles and fractured bones remaining as causes of irritation. The wounds presented an angry and sickening appearance, discouraging to both patient and surgeon.

After arriving there, the wounded were removed to a hospital. I was put in charge of the hospital. Luckily, I was in possession of a complete case of surgical instruments. The dressing material (bandages, &c.) were furnished of a quality and quantity of which I could not complain. Next day after our arrival, April 16th, assisted by a resident physician, I commenced operating. Chloroform was administered, and while under its influence divided the scalp sufficiently to get to the extent of fracture. The loose pieces of bone were carefully removed, and the rough edges of the margin of the bone pared off with a small chisel made for the purpose. In removing one small piece of bone, the middle meningeal artery was unavoidably ruptured; the hemorrhage, however, was readily controlled by a small piece of lint, closely pressed, with a probe, over the end of the artery. The scalp was closely shaven before commencing the operation.

The flaps of a scalp where the destruction of parts was not too great, were brought together with silken sutures. Small strips of isinglass plaster were then applied across the wound, to keep the edges in proper apposition. April 19th the wound was dressed for the first time. Suppuration had fairly commenced, and the symptoms were all good. The wound was kept clean, the patient's bowels regular. Sometimes it was necessary to give an opiate. In about four weeks the patient had entirely recovered, with no bad symptoms following.

Case 2. Frederick R., belonging to 43rd Ills. Vols., was hit near the *posterior superior* angle of the *parietal* bone, fracturing the parts more extensively than in case first. The patient was

operated on in the same manner as the first: the loose pieces of bone carefully removed; the flaps adjusted: three or four days' water dressing was applied. Subsequently the wound was cleansed with warm water and castile soap. The patient's bowels attended to; opiates more frequently administered than in case first, on account of more irritation and general excitement of the patient. The cure in this case was more tardy than in the former, from the greater extent of the injury. In about three weeks a cartilaginous substance commenced to be deposited in the space from which the previous bones had been removed, which gradually became more ossified, and in about six weeks the wound was entirely well.

CASE 3. Jasper S., private of my Regiment, 18th Mo. Vols., was hit by a musket ball, back and a little above the *frontal eminence* of the *os frontis*, the missile striking the bone lengthwise, depressing it the length of the ball, and half an inch depth. The man fell, was insensible, and conveyed in that condition to the hospital for surgical treatment. He was examined by several surgeons, and at one time it was determined to use the *trephine* to remove a portion of depressed bone. That opinion was finally overruled, and we came to the conclusion to take the conservative course, and trust to the powers of the system. The man was put in charge of a trusty nurse, his bowels attended to, quieted with opiates, trusting to the brain to adapt itself to the condition of the skull. Gradually the man became more rational, and in less than three weeks he had recovered his usual health. Since my return from the army the man has called at my office,—says he has had no trouble since his first recovery. He has gone to his family.

There were many other cases of fracture of the skull among the wounded under my charge. The fractures were of various characters, some indentations the size of the circumference of a musket ball, with the plug of bone driven in on the brain. In this class of cases the chisel and elevator, or lever, were the instruments used, the depressed bone carefully removed, the rough edges pared smooth, the patient kept quiet, bowels attended to, &c. They all made good recoveries.

My impression is, from what I saw in other portions of the army, that, had some of the cases been subjected to the *trephine*, as suggested, would have terminated fatally.

In fractures and other injuries of the skull, I believe in conservative surgical interference. There are undoubtedly cases where the *trephine* ought to be used, but that it is resorted to in many cases without due forethought and judgment, I have reasons to believe. I have seen many patients trephined, and most of them subsequently died, when, I believe, had they been left to the efforts of nature, assisted by art, would finally have recovered.

REMARKABLE RECOVERY FROM GUN-SHOT WOUND.

BY J. A. ELSTON, M. D., ELSTON STATION, MO.

Samuel Cowan, aged fifty years, received a severe gun-shot wound on the 18th of September, 1856, in the following manner: Wishing to ascertain whether his rifle was loaded or not, he placed his foot upon the hammer and his mouth over the muzzle of the gun, to blow into it, when his foot slipped from the hammer and the gun was discharged, the ball, of large size, striking and carrying away two of the incisor teeth, also a portion of the superior maxilary, the vomer, and all the nasal bones, making four incised wounds through the soft parts of the nose between the eyes; striking the os frontis centrally, and fracturing that bone into a number of pieces, separating it from the parietal bones at the coronal suture entirely. Saw him an hour after the accident; found him partially sane, and bleeding profusely. I passed probe up from wound at the root of the nose, and found large piece of loose bone; extracted it with the forceps, and found it to be a portion of the superior maxelary, 1½ inches long by ¾ of an inch wide. Smaller portions were removed, but I failed to discover the ball, although I passed the probe as far up as the coronal suture. I placed the fractured pieces of the os frontis as nearly

as I could in their normal positions, and applied the bandage; directed cold applications applied continually to the head, and a cathartic. Saw him next morning; found the face and eyelids much swollen and red, but there was no injection of the conjunctiva, and he answered my questions intelligently. Pulse about normal; bowels had moved in the night. Continued the cold applications to the head. The weather being very warm I was in much fear that I would lose my patient by hemorrhage, when suppuration began, which necessarily would be extensive. But no severe bleeding occurred, and there was no inflammation of the brain; but about the twentieth day from the accident, the patient began to show signs of pressure on the brain. He became very stupid, and his bowels much constipated. Found the bones of the os frontis more loose and crepitating, and considerable accumulation of pus. I made an incision about the frontal eminence, and there was discharged about two ounces of pus, but with little relief to the patient. It now became evident that my patient would die if the pressure was not removed. I called in my brother, Dr. A. Elston, and we concluded at once to remove the dead pieces of the os frontis. I consequently made an incision about an inch above the root of the nose, and carried it back nearly to the coronal suture. Considerable amount of pus was discharged, and we removed about one-fourth of the entire bone. Found and extracted the ball, $\frac{3}{4}$ of an inch long by $\frac{1}{2}$ inch in diameter, pressing against the pia mater and upper edge of the crista galli, having passed entirely through the os frontis, breaking up the frontal and longitudinal sinuses. The edges of the wound were brough nearly together with adhesive strip; instructions were given to remove the bandage from time to time, that the pus might escape. From this time on he grew rapidly better, and I closed the wound with sutures, on the seventh day after the operation, and about the sixtieth day from the accident he was able to ride on horseback to Waynesville, Mo., near which place he now resides, and is able to follow his trade (gunsmith).

Out of a number of cases in which the injury was received in this way, this is the only one that I have known to recover.

Correspondence.

CHILDREN'S HOSPITAL—M. BOUCHUT.

SYPHILIS IN YOUNG CHILDREN.

[From the Notes of Mr. Duguet, *Interne du service.*]

PARIS, Aug. 20th, 1865.

MESSRS. EDITORS:—The summer session in the School of Medicine here has just closed, and now we are enjoying a little resting spell. The Professors have nearly all gone to the country for a few weeks' recreation, and when refreshed, will return and again commence the noble work of "Teaching how to live." Most of the students are leaving to enjoy a few weeks' repose, and now the "beau Quartier Latin" will for a time lose its accustomed life and bustle.

Among the many interesting "*cliniques*" that I have visited during the last session, that at the Children's Hospital, conducted by M. Bouchut, has offered no few interesting facts. The following lesson that I listened to some few weeks since, from the lips of that able professor, present many points of interest, and I give it to your readers, as reported by M. Duguet, for the *Gazette des Hopitaux:*

"It is not often that congenital syphilis is seen in the wards of the Children's Hospital, for, according to the regulations, a child must have reached the age of two years before it can be admitted. Now, it is well known that congenital syphilis affects, ordinarily, newly born children. I say ordinarily, for there exist examples where it has been seen in children at the age of five, seven, ten, and even fourteen and fifteen years. My friend, M. Ricord, has seen cases yet more uncommon. I speak of two brothers, in whom hereditary syphilis did not make its first appearance until the age of forty years.

"It requires at least such an imposing authority to have the right to assert such exceptional facts.

"For my part, I saw, some few years since, at Hospital Sainte Eugenie, a little girl ten years of age, whom I showed to M. Ricord, and in whom hereditary syphilis had just made its appearance, by perforating the partition of the nasal

fossæ and destroying the palatine arch. The learned syphilographer did not hesitate to recognize in this, that form of tardy syphilis of which I speak, and the treatment has entirely confirmed his opinion.

"But let us return to the ordinary cases in medical practice.

"Now, although we rarely see the accidents of constitutional syphilis in the wards, yet, at the consultation, facts of this nature are presented to us at every instant.

"The children that are presented to us there, are, the most often, four, five, and ten months old. The diagnosis of the disease is not always simple nor easy, and you understand how much this question, not only for the Hospital, but yet and above all, for the city, possesses a very high practical interest.

"I have had brought before you, three little children that were presented to us at the consultation, offering accidents of a different form, in order to familiarize you with this disease, and to serve as a base for our clinical conversation of this morning.

"The first is a child nine months old; when he came into the world, so its parents tell us, he was small and sickly. Was he at term? That is possible; but the mother cannot tell the precise date of the commencement of her pregnancy, considering that the catamenial flow was regular during all the time of gestation as it was before. If it be before term, it would not be surprising, and that would be a sign of syphilis, since abortion is the rule in these conditions.

"However that may be, this child, given to a nurse as soon as it was born, passed two months in an apparent excellent state of health; but at the end of the second month, the nurse saw appear upon the buttocks and upon the genital parts, some kind of boils or furuncles which opened themselves. Around these furuncles the skin became red and inflamed, and there took place upon this level, a fall of the epidermis by scales; afterwards the feet and the calves of the legs were the seat of small bullæ, such as are formed by a blister—in a word, the bullæ of pemphigus. After these bullæ, followed dry crusts or scabs, after the fall of which, there appeared no more of the phlyctenæ. Upon the face and the chin, behind the neck, and especially about the mouth and nose, the skin became equally as red; and this also passed off by scales.

"It was possible to state a moment of arrestation in the march of these accidents, and the child seemed almost cured without treatment, when, fifteen days since, these accidents again made their appearance, and now have reached progressively that state which you can study to-day.

"In the first place, you are surprised to see a child nine months old who has no teeth; and in whom neither any of the sutures nor the fontanel are closed, at an age when only the anterior fontanel should not be closed by ossification.

"Moreover, the chondro-costal articulations present nodosities which admirably represent the beads of rachitism (le chapelet rachitique); the osseous extremities of the long bones are also a little larger that usual. We have, then, all the attributes of rachitism.

"But is this rachitism primitive or secondary? Has the syphilis exercised (yes? or no?) a direct influence upon its production? This is a highly interesting point, and we will return to it a little further on.

"Besides this rachitism, which we lay aside for the second part, you have under your eyes one of the most beautiful examples of secondary syphilitic eruption to be found in the newly born child.

"In fact, around the orifices of the mouth and nose, which are covered with crusts or scurf, which are fissured, and offer the radiated aspect of a purse when the strings are drawn, the derma is glossy, deprived of its epidermis, of an intense copper color, or of the color of smoked ham; within these bounds, upon the middle of the cheeks, and following some denticulated curves, the chinked epidermis detaches itself, from whence that peculiar aspect of a cockade (cocarde) to which M. Natalis Guillot attaches a grand semeiotic value.

"Upon the rest of the face, below the chin, upon the back of the neck, there exist red projecting points, spread out circularly, depressed and often ulcerated in the centre, but on the contrary elevated at their periphery. A few of these flat disseminated tubercles are seen upon the body. Upon the vulva, and in particular upon the right labia majora, the tissues are very much tumefied, and still bear the traces of two or three recent pustules. Around the anus the mucous spots (plaques muqueuses) are very confluent, and let ooze a grayish liquid with a strong odor; and in advancing towards the buttocks, the skin presents numerous ulcerations with a kind of eczematous desquammation. The thighs and calves are equally covered with flat tubercles, united frequently in almost circular groups.

"Let us note that the buccal mucous membranes, nasal and pharyugeal, seem to be healthy. Moreover, the child sucks sufficiently, and although he has at times a little diarrhœa, he is far from perishing.

"Let us add, in a word, that neither the father nor the mother, nor the nurse, give us, in their state of previous health, the original cause of this disease of the child. There exists there an unknown cause that is not for us to discover.

"The second child is four months old. When he came into the world he was apparently healthy, but six weeks after, there appeared an eruption upon its body, which you can study to-day in all of its forms. It is a papulo-pustulous syphilide, which is found diffused upon the face, the superior and inferior extremities, the body, and especially about the anus and the genital organs. The anus is entirely occcupied by oozing mucous spots (plaques muqueuses), forming a pad or cushion (bourrelet), which spreads out upon the internal face of the buttocks, in all of the extent of contact of these regions. In the inguinal fold are found the same particularities.

"Let us observe here that this is the first child, and that the mother remarked that, sometime after she was married, she was covered with pimples and crusts, which extended even into the hair, which has fallen in consequence. This simple information should serve us as a light.

"The third, in fine, although it does not present us an eruption so multiplied or so confluent as the other two, does not lack for interest on that account.

"There is in the palms of the hands a form of syphilide which is far from being common in the young child. There are losses of substance of the epidermis, of a rounded form, as if cut out in places by a punch, and occupying only the palms of the hands, and, to a small extent, the soles of the feet and the heels. Upon a

level with the point where the epidermis is detached from the derma, this latter is of a red copper color. The child does not seem to suffer from this eruption.

"In interrogating with care the parents, who have watched the child from the commencement, we learn that the fall of the epidermis has never been preceded by phlyctenae or bullae; that the hollow of the hands has always been dry and without any protuberances. There is, then, a veritable syphilitic palmar psoriasis, such as is seen in the adult, but with this difference,—that it does not appear as neatly formed by the punch, and has not, as in the latter, an aspect so characteristic.

"This child, which is six weeks old, is also covered with copper-colored spots, particularly upon the arms and thighs. It is the fourth child of the same bed. The two first came into the world dead, at seven months; the third came living, at the seventh month, but died seven months after birth, covered with spots and pimples. It is, besides, well proven that the father is infected by syphilis.

"These three examples resume, in a manner sufficiently complete, the different syphilitic accidents that you can meet with at this age, and a description of which you will find in my work on diseases of children. I do not speak of the chancre, since, if it exist in the fœtus, where, how, and following what evolution? These are many questions that are as yet far from having any solution whatever. Neither do I speak of tertiary accidents; they are presented to us much more frequently under the study of pathological anatomy than as a symptomatic study: let us but remember the alterations in the liver, so well described by M. Gubler; in the lungs, by M. Depaul; in the thymus gland, by M. P. Dubois: alterations which generally explain the death, *in utero*, of the fœtus, and coincide frequently with eruptions of pemphigus.

"Hereditary syphilis of young children, presents itself to us, then, under the form of secondary accidents, characterized by spots, scales, papulæ, or flat pustules, upon the skin and mucous membranes, particularly in the commissures of the natural orifices. As to the pemphigus, over the nature of which there still hover some incertitudes, but which, in the majority of cases, is an incontestable sign of syphilis,—it can exist at birth, or may show itself as the other accidents, some time after, that is to say, near the sixth week.

"Join to this some general phenomena, characterized, at the end of some time, rarely so at birth, by a profound anemia, with decoloration of the tissues, a veritable syphilitic cachexy.

"Such is the case with our third child. Is its enteritis the cause of its cachectic state, or rather is the enteritis itself, as M. Cullerier willingly believes it, is it not also a manifestation of the syphilis? We should regard the two as possible, for the science embraces similar cases, in which mercurial treatment has succeeded. But as the question is not entirely decided for us, we shall give conjointly, but at different intervals, the mercury and the sub-nitrate of bismuth.

"It is easy to understand how deplorable it would be to confound the syphilitic eruptions with scrofulous or herpetic manifestations.

"The eczema alone, as a scrofulous eruption, might deceive; but yet it is a rare affection as an elementary form, and seldom shows itself about the anus and the genital organs. Besides, it is not characterized by a red, copper-colored hue, by

shining spots with radiated fissures, and terminated at the periphery by a border of epidermis in a state of desquamation.

"It would be the same for herpetic eczemas and impetigo.

"But there is an affection with which a confusion would be much more easily made; it is the entero-colitis, with an acid secretion, a secretion which so often gives rise, in the young child, to an erythema of the buttocks, with ulcerations extended to the thighs, to the legs, and to the malleoli; and in cachectic children, these ulcerations take a brownish coloration, which could be mistaken for constitutional syphilis.

"Such is the case that was presented to us eight days since, at the consultation. It was embarrassing; the word syphilis had already been pronounced in the city, and we had before us a father, a mother, and a nurse, to whom would have been given some trouble, if our answer had been in the least affirmative. And it was not until after a very close examination of the little patient, after having seen that there was nothing the matter with the throat, nothing upon the face, nor upon the rest of the body; that, in a word, it had had the diarrhœa for a long time, that we believed ourselves right in rejecting syphilis. And, in fact, in eight days the sub-nitrate of bismuth, *inters et extra*, has almost entirely cured it. The practice is full of examples of this kind, and I choose but one in a thousand.

"After all, when, six weeks or two months after birth, there is seen to appear upon the child hitherto healthy, bullæ upon the extremities, flat pustules about the anus and upon the mucous membranes, and a general eruption of a copper-colored red, or of the color of smoked ham, it is right to believe that you have to do with a constitutional syphilis.

"Join to this the antecedents, when you are happy enough to discover them without the risk of injuring in the least the parties interested. It is our duty not to throw trouble where sweet illusion and peace reign; and we must think that the nursling would be the first victim.

"The effects of syphilis do not always stop there; witness the first child, in whom I called your attention to the tardy development of all of the osseous system. At nine months it has no teeth. All of the sutures and fontanels are open; the spongious extremities of the long bones are knotty, as also the chondro-costal symphyses. It is a rachitism resulting from syphilis, such as every chronic disease might produce;—rachitism acquired, which some authors are not willing to admit, but which should, according to us, find a place by the side of congenital rachitism, which is hereditary. Is it not easy to understand, in fact, that a constitutional affection which acts upon a child for seven months, may arrest in it the nutrition of the bones.

"But let us continue. What has been here—what is in every case—the real origin of syphilis in the young child?

"This question, full of obscurities and sometimes of clouds, as we have just said, has at last found a complete solution. The child can be infected from three sources—that is to-day superabundantly demonstrated,—the father, the mother, the nurse. From the father and mother, by an influence that I shall call direct, viz.: the sperm or the maternal blood; from the nurse, indirectly, by transmission of the secondary accidents.

"As to our first patient, the mother has neither the leucorrhœa, nor any pimples or furuncles about the genital parts, nor any eruption on the body; the father is said to be in perfect health. As to the nurse, she is as healthy as the mother. Let it, then, be sufficient for us to state a syphilitic eruption by its characters, syphilitic also by its manner of appearance in a child otherwise healthy, at the second month, though I shall always remember of having seen a child born at 'la Pitie,' in the service of M. Michou, who was affected with pemphigus upon the extremities, and syphilitic onyxis upon all of the fingers and toes.

"In our second child the disease seems as if to have come from the mother. We do not wish for a proof, besides the eruption with which she was affected, but the repeated abortions, as is so often seen in city practice, abortions which take place for many years if a remedy be not employed, and thus cause many women to be incapable of bringing to term a living, healthy child. Everything is accused as a cause, but in vain, and, as a last resource, the physician criminates syphilis. Thus let the woman, after several abortions, submit, even during pregnancy, to a mercurial treatment under a form more or less disguised, and she finishes by bringing to term and giving birth to healthy children.

"To-day, in fact, it is admitted that the mother can give birth to syphilitic children, not only when she has taken the disease during her pregnancy, in such a manner that the child takes from the chancre upon its passage, the origin of the disease which makes its appearance six weeks after, but yet during a variable period, difficult to precisely name.

"Our third child seems to have the syphilis from its father. Now, it has not been long since that the power of transmitting syphilis to the child has been attributed to the father. This doctrine was not sufficiently convenient for the partisans of the chancre; but we must say, that at this epoch, the ideas are entirely changed, and now one is inclined to exaggerate the role of the father in this transmission.

"It is, then, demonstrated that the semen can contain syphilitic properties; and why should it not be thus, since we see, every day, without being able to better explain, the epilepsy, the scrofula, the character, &c.,—in a word, all the moral and physical resemblances transmitted by the semen.

"This is not all, for not only does the semen give syphilis to the germ, but then the germ can give syphilis to the mother; this results from the impregnation of the woman by the man. Man holds dominion over woman; a widow who remarries may give birth to a child that has all of the characteristics of the first husband, and comparative physiology shows us that a zebra will get, with a mare, a mule-zebra, and that the same mare will have another mule-zebra by a horse.

"If the father and the mother can transmit syphilis to the germ, the germ will transmit the syphilis that it has from the father, to the mother; the mother will transmit to another healthy germ the syphilis that she has by impregnation from her first husband; the child can also give it to its nurse, and the nurse can give it to other suckling children.

"The child, we say, can infect its nurse. Twenty years ago, facts of this nature were habitually denied, and the syphilographers rejected them because, according to them, where there was no chancre there could not have been any possible

transmission of syphilis. I was the first who attempted to reform this too exclusive opinion, by a work inserted in the *Gazette Medicale*, and now it is generally admitted that syphilis may be transmitted from the child to the nurse, without there being a chancre.

"The practical importance of thus regarding it, shows itself. Be it what child it may, it will not be hereafter trusted to a nurse, as has been done in the past, otherwise one would run the risk of giving syphilis to the nurse; and it is easy to foresee the consequences in looking at it in a legal sense.

"This is not all. A nurse, having been affected without her knowledge, can give the syphilis, as she has received it, to other children. As is known, the true syphilitic accidents (plaques muqueuses, pleiades axillaires, &c.) are not painful; the nurses, not being preoccupied by a disease in appearance so benign, believe themselves capable of taking other nurslings without there being any danger. There is a fact that I have seen at the Direction of Nurses, street Sainte-Apolline, which I shall ever remember. It is that of a woman, who in ten years affected, by three of her children, three nurses of the municipal administration. At the moment of their arrival at the age of two or three days, when they were presented at the visit of the physician, the children seemed to be in perfect health; this was attested by a certificate, and they were entrusted to a healthy nurse, who took them into one of the provinces. Now, two months after, out at nurse, far from the city and the administration, syphilitic accidents showed themselves in them, and the foster-mother was infected with syphilis, which, to complete her ills, she transmitted to her own child.

"It was not until the third time that the administration perceived what had arrived, and then because of the damages and interests it was obliged to pay to the infected nurses, and the same name having attracted their attention, they were obliged to refuse a child that the mother, ignorant of her state, brought to them again.

"Such an example speaks for itself, and I shall not dwell longer on it here.

"What, now, is the initial accident by which a nurse takes a chancre from the child? It has been stated that the mumbled nipple ulcerated and became the seat of a chancre. The axillary glands would then announce it; but the majority recognizes about the nipple, in this case, only the mucous spots. What is to be done for this hereditary syphilis? The treatment is preventive and curative.

"Preventive when, before a new conception, because of anterior abortions the mother is submitted to a form of anti-syphilitic treatment. Preventive still, when for the same motive, during a new pregnancy, is instituted a similar treatment, which, to be efficacious, and not dangerous to the fœtus, has need of extremely close attention; for it is known that mercury easily kills all product of conception. One will have for a guide, almost sure in this case, the more or less tolerance of the digestive organs of the mother.

"Curative, in fine, and there direct or indirect. Direct, in giving to the child itself a teaspoonfull of the liquor of Van Swieten a day, and in plunging it every day into a wooden bath-tub containing the following solution:

Corrosive Sublimate.....	2 grammes, or xxxi grains,
Alcohol.....	2½ scruples,
Water.....	q. s.

and during the interval in using frictions of mercurial ointment in the axillary regions and in the inguinal fold. If it were impossible to give internal medication, it would be easily seen what advantages would have resulted from external medication. In every case the gums should be examined with the greatest care, although in the absence of teeth, salivation may be a rare phenomenon.

"I am not a partisan of the indirect treatment alone; I find it excellent for the mother, above all because of future pregnancies, but not for the child that she nourishes; for is one very certain that the milk can be charged with mercury in a sufficient quantity to affect the child? The milk, in fact, contains albumen, which could form, with the mercury, even very much divided, an incompatible mixture.

"However it may be, care must be taken not to give to a nurse, without forewarning her of the dangers that threaten her, a child born in certain conditions, although at its birth it be the most healthy looking child in the world. Let the child be given to a nurse who, being acquainted with the facts, still wishes to take charge of it; or, better, let the mother herself nourish her child; but do not put to the sucking-bottle a child already cachectic; that would almost surely lead it to its grave."

When the winter session opens I shall try and find something more for your Journal. Wishing a continued success,

I am truly yours, etc.,

R. H. HAYNES.

SYPHILITIC MALADY.

A collection of Notes on the origin of Syphilis, the different Doctrines, &c., maintained by the different European Authors—embracing a few questions to M. Ricord, with his Reply, &c., &c.

BY DR. A. C. HAYNES.

A great number of opinions have been advanced upon the plurality of venereal maladies. Some syphilographers, the identistes, following the errors of the doctors of the two last centuries, profess to believe that syphilitic infection can begin indefinitely by blennorrhagia (gonorrhœa), the simple chancre (chancre mou, chancroid, chancrelle), and the chancre proper (chancre induré,

chancre infectant). The non-identistes admit that blennorrhagia is never followed by constitutional syphilis. They divide themselves into unicistes, who believe in the identic nature of the simple chancre and the infecting chancre; and into dualists, these last pretending that an infecting chancre is always the first symptom of the pox, and that the simple chancre, on the other hand, has no connection with it, being an ulceration of contagious nature existing from all antiquity, whilst syphilis is of modern origin. To this opinion the theory of *hybridity* attaches itself. M. Clerc, who is the author of it, states that the chancroid (simple chancre) is a derivative of the chancre, but that it propagates itself in its own species.

IDENTISTES.

The doctrine professed by the identists, has, during several centuries, reigned supreme; yet the first authors who wrote upon the pox, and amongst them Alexander Benedictus, and Marcellus Cumanus, did not confound syphilis with the simple chancre and blennorrhagia.

It was about 1530, that all distinction ceased between these three diseases, and the theory of identity originated, which was adopted without reserve by the doctors of the sixteenth, seventeenth and eighteenth centuries. In 1741, Van Levietere, in his commentaries upon the aphorisms of Boerhaave, pretended that gonorrhœa had no connection with the urethral flowings described by the ancients.

Admitting the American origin of syphilis, he attempts to prove that genuine gonorrhœa, which he considers as a syphilitic manifestation, had appeared for the first time at the epoch of the famous epidemic of the fifteenth century.

Astruc, adopting the same ideas of the origin of pox and its identity, attempted to explain why the syphilitic virus did not produce constantly the same primitive manifestations. He pretended that a chancre resulted from a contamination by superficial moisture, whilst exhalation produced gonorrhœa, and, finally, that absorption was followed by bubo.

Sanchez, so different from Astruc, in his opinions upon the origin of syphilis, thought that it could begin either by chancre or gonorrhœa. Daran, extended the limit of the primitive symptoms of the pox, by including les flueurs blanches, which opinion was disputed by the majority of practitioners.

The celebrated J. Hunter, whom the identistes were proud to count in their ranks, was hardly convinced upon the subject, since he wrote that gonorrhœa is followed but one time out of a hundred by constitutional syphilis.

Cullerier, whose clinical teachings had so great a success in the first years of this century, admitted the doctrine of syphilitic blennorrhagia, but at the same time believed in the existence of another blennorrhagia of contagious nature, not syphilitic.

In our enlightened days, the doctrine of identity has no longer any representatives. MM. Cazenave, Devergie, Gibert, and the much regretted Vidal (de Cassis), have battled well, for a long time, against the non-identity of the poisons; which doctrine has triumphed, however, in spite of all their efforts to the contrary. Amongst this number I might also mention M. Velpeau, a man whose name is respected in the four corners of the globe.

NON-IDENTISTES.

The doctrine of the non-identity of the blennorrhagic and syphilitic poisons, was promulgated for the first time by Belfour; and some years later, in 1777, by Prof. Toel, of Copenhagen, in a small work, which stirred up the most violent opposition to the views of the latter.

Bosquillon, the commentator of B. Bell, advanced that pox had an American origin, whilst gonorrhœa, on the contrary, was well known to the ancients. He is also the author of the following important fact, to-wit: that gonorrhœa always gets well without mercurial treatment. The medical world, instructed by experience, which speaks louder than all theories, accepted without difficulty the ideas of Bell and Bosquillon. The Medical Society of Besancon put to the "concours" a question, presented in the following order:

"To decide, by experiments and conclusive observations, whether there exists any natural identity between the virus of virulent gonorrhœa and that of pox; if the one can give the other, and if the treatment which is appropriate to one is applicable to the other." A distinguished doctor, M. Hernandez, surgeon-in-chief of the navy, and professor of the School of Medicine, of Toulon, sent to the "concours" a very remarkable memoir, which took the prize, and which is the most satisfactory exposition in favor of the doctrine of non-identity:

1st. "By proving, from historical researches, as did Bosquillon, that syphilis and gonorrhœa did not appear at the same epoch."

2nd. "By proving, experimentally, from numerous inoculations, that the virus of gonorrhœa never gives place but to gonorrhœa, and never produces chancres."

3rd. "By showing that gonorrhœa is never followed by constitutional infection." It is in this third part of the memoir that we find put forth, for the first time, the hypothesis of the concealed urethral chancre, in order to explain the rare cases in which gonorrhœa transmits a chancre. This hypothesis has since received (thanks to the works of M. Ricord) a complete confirmation.

We ask how, these three points being proved, the old doctrine of identity did not immediately succumb? Indeed, without stopping to examine the historical arguments invoked by Hernandez, and only occupying ourselves with clinical facts which he has advanced, we will ask this question: How is it possible to reunite and to confound two diseases of which the symptoms are so different, and which produces, in no single instance, the one from the other; the first being always a local affection, the second a constitutional affection? The constant negative results from the inoculations practiced by Hernandez, and later by M. Ricord, strictly suffice to establish the three points just considered.

UNICITY.

Unicity, is the doctrine of pure non-identity; it is one which the school of Midi has endeavored to propagate. Its parti-

sans believe in the identity of the infecting chancre and of the simple chancre.

The syphilitic virus, according to them, produces a local chancre (simple chancre), or an infecting chancre, depending upon the particular idiosyncrasy of the individual contaminated. This theory, with the exception of that one which makes blennorrhagia a source of infection, is, nevertheless, nearly as ancient as syphilis itself. At a period when the existence of the lymphatic vessels was not known, the phlegmonous bubo was regarded as a favorable crisis. Nicolas Massa wrote that these bubos were species of emunctories, which rejected the virus and purified the system. But we know that the phlegmonous bubo is always, or almost always, a symptom of a simple chancre. Thierry de Hevy says, page 17: "And if a bubo follows it,—in other words, a swelling in the groin,—the ulcer will be healed, and will exempt the patient from syphilis." Ambroise Paré also says that the virus infects the mass of blood, if the boils do not cast off their cores.

After the discovery of the lymphatic vessels, Astruc and Petit-Radel, in order to explain the non-infection in certain cases, and the infection in others, asserted that the inflammation of which the chancre is the seat, prevented the penetration of the virus into the organism. This interpretation is not admissible, for oftentimes a case where the simple chancre is not inflamed, is followed by a bubo, and yet is not infecting.

Vacca Berlinghieri, in 1800, pretended that the ganglia have the power of arresting the penetration of the pus; he also considers the phlegmonous bubo as evidence of the arrest of the syphilitic virus, which is then eliminated by suppuration.

[To be continued.]

MILTON, ILLS., Jan. 1, 1866.

Prof. M. L. Linton, M. D.,
St. Louis, Mo.:

MESSRS. EDITORS—Every species of humbug is called *progression.* Delusions are common in medicine, in politics, and in religion. I do not claim for the medical science, exactitude, but progress. If their mystic wonders are incomprehensible, the best minds are engaged in their solution.

The honor is yours of setting aside the errors of ancient nosologists—of establishing that disease cannot readily, if at all, act on the organism, except through the blood; of drawing a distinctive line between original and consequential disease,—between lexion of structure and derangement of function,—between disease and its symptoms.

The great barrier to science is, that some observers are too theoretical; others are careless as to theory, but blindly plod the practical detail of some antiquated adage or exploded dogma,—indeed, excelling the monkey in imitative genius. If they exhibit any sign of life, it is in promulgating the errors of the last generation, with a zeal that would accomplish anything but promote science or make error useful.

Allow me to assert my belief that the practice of medicine, as sanctioned by American and European schools, *is too anti-phlogistic.* If ptyalism, active blood-letting, antimonialization, or the withholding a moderate portion of nutritive food from bed-ridden patients is justified by any established fact in pathology, it is my misfortune that I have never had the erudition or the acumen to perceive it.

It is known to you that I was born into the profession bearing upon my lips a firm remonstrance to the anti phlogistic regimen.

Nine years' extensive practice, civil and military, has well confirmed the correctness of the principles which guide me, and justifies me in saying to the profession, and particularly to those who are novices in professional life,—be careful to avoid active

depletion, but assist the vital powers by every means not positively contra-indicated.

Charlatans often get advantage, by doing nothing, over anti-phlogistic doctors, who often do absolute harm. This exposes the profession to ridicule, and makes skeptics of the people, who are justly entitled to act as umpires.

Thanks, in behalf of the profession: reformation is going on, for which we are much indebted to Prof. Bennett, of Edinburg.

As politicians may be more radical than union doctors, and more theoretical than scientific, yet, without endorsing all that Dr. Bennett has said, it will be admitted that he has expressed some ideas that are new, and many facts that are useful.

To exhibit the practical good which reformation has accomplished, we will take, for example, Pneumonia, for which the treatment recommended a few years ago would have well nigh killed a well man. M. Louis was very anti-phlogistic, and the mortality among his pneumonic patients was 1 in 3½. M. Grisalle was a little more moderate, and his success correspondingly better: he lost 1 in 8. Balfour (homeopath) reports that he lost 1 in 13. If his statistics are worth anything, it evinces that pneumonia had better be let alone than treated actively; for every physician knows that his "*infinitesimals*" never influenced disease in any way.

Bennett lost 1 in 40, and of uncomplicated cases he lost *none*. He depleted none, but sustained all. These results disprove the former doctrine that depletion is necessary to cut short the inflammation and prevent the lung from destruction, and they strongly corroborate the pathological deductions which suggested the practice, viz: He holds with you and other modern pathologists, that a stasis of arterial blood is inflamation. He treats for resolution by regulating the formative power of the exudation, which undergoes cell-transformation; the cells break down and are absorbed into the blood and are taken out by the emunctories. This process is to be aided by counter irritants, warm fomentations, calmitives, diuretics, diaphoretics, moderate purgation,

moderate stimulation, and nutritives. Instead of depletion, the vital powers are to be sustained.

We have all seen that inflammations are far more persistent and dangerous when they occur in feeble patients than in the robust.

Now, if a sustaining treatment is admissible,—indeed, absolutely demanded in inflammatory and sthenic diseases,—then what about the asthenic and the zymotic fevers?

I have for many years treated pneumonia substantially on the principles elucidated by Dr. Bennett, and without any preserved statistics. I assert my opinion that my loss has been less than one in fifty. May the profession not honor itself by abandoning a practice the ill success of which disproves the correctness of the principles upon which it is founded.

C. L. CARTER.

Transactions of Societies.

PROCEEDINGS OF THE ST. LOUIS MEDICAL SOCIETY.

REPORTED BY JAMES W. CLEMENS, M. D., RECORDING SECRETARY.

CHLOROFORM IN PARTURITION.

Dr. Bland wished to hear the opinions of the members of the Society in regard to the use of chloroform in midwifery. For the first fifteen years of his professional life, he had been without chloroform, and that period had been one of anguish both to himself and patients. Since his use of chloroform in that particular branch of his professional duties, he had been enabled to

ameliorate, to a very great extent, the sufferings and anxieties which are incident to the lying-in condition. He had administered it very recently to a lady, who had not taken it in her previous confinements, and she passed her labor with great ease and comfort, and thanked God that the Doctor had used chloroform. He never used chloroform until true labor pains had been developed. Many pains do not further labor—they are sharp and lancinating, not those forcing and bearing down pains which expel the fœtus. The patient becomes restless and anxious, tosses from side to side, and complains that she is not advancing. These are not true labor pains. To corect this condition he was accustomed to use cathartics, opium or the lancet. He would give forty drops of laudanum or two grs. of opium to quiet this condition; and sometimes in the sleep which was induced, true labor pains would come on, which would soon relieve the patient. When labor pains are true, they are bearing down, pressing the fœtus to the external parts, and then is the proper time for chloroform. It should be given during the pains and taken away during the intermissions. When the os is dilated or dilatable, and the pains bearing down, he would not hesitate to use chloroform. In turning or in the use of the forceps, if the contraction of the uterus was great, he would bleed to syncope; and he had often known, after this had been obtained, that the hand would enter easily where it was impossible to introduce it before.

Dr. Hammer said, that he was the first to administer chloroform or ether in Germany, in obstetric practice, and, at the time, was much censured by the older physicians and clergy. He had at first used ether, which, although not so pleasant as chloroform, yet, he thought, was safer in its action. Both exerted the same kind of influence over labor; neither of them lessened the force of uterine contraction, but rather abridged the period of labor, whilst they relieved the patient of the intense suffering she would otherwise be compelled to pass through. He found that there was a great difference between the labors of the women in America, and of those in Europe; the former being much shorter, frequently terminating in a few hours, while the latter seldom terminated in

less than twenty-four or forty-eight hours. He confined himself, in his use of chloroform, to any period *after* the *first stage* of labor had been *completed—i. e.*, when the os uteri was fully dilated and the membranes portruding. He had more frequently used it when the child's head was in the inferior strait, than under other circumstances. He would not give it in a case where the pains had ceased and the labor apparently suspended; but would then resort to such remedies as would tend to re-establish labor; nor would he use it if the patient was affected with disease of the heart, especially if fatty degeneration was suspected.

DR. JOHNSTON was in the habit of waiting until the labor was considerably advanced, and did not aim at more than a perceptible influence at first, and then gradually increasing it as labor advanced towards its termination. The accidents most to be feared were congestion of the brain, lungs or heart; and he thought that its administration might in some instances, induce a typhoid condition after labor, with a tedious convalescence. He cautioned against its use merely to relieve nervousness or modeate pain. He begged leave to read the following paper:

MR. PRESIDENT.—It is strange that we have among us, at this day, obstetricians in full practice, who refuse to use chloroform; but such, nevertheless, seems to be the fact in our city and country. That such should be the case with so many facts, with the full records of cases published to the world, is so unreasonable, that we cannot comprehend or understand why it is so. Of all remedies of the "Materia Medica" which have been brought into use to relieve suffering humanity, chloroform stands first in the rank of remedies; and we hold it to be our duty to use it in all cases of confinement, unless the situation of our patient forbids it.

It will be well worth our most serious consideration, in regard to the employment of chloroform in our obstetrical practice, to consider attentively the report of the Committee, made to the "Royal Medical and Chirurgical Society of London." The following quotations are most important:

"*Physiological Conclusions.*—The first effects of chloroform vapor is to increase the force of the heart's action; but this effect is slight and transient, for when complete anæsthesia is produced, the heart in all cases acts with less than its

natural force. The strongest doses of chloroform vapor, when admitted freely into the lungs, destroys animal life by arresting the action of the heart; whilst, by moderate doses, the heart's action is much weakened for some time before death ensues; respiration, generally, but not invariably, ceasing before the action of the heart; death being due to the failure both of the hearts action and to that of the respiratory functions. The danger attending the use of chloroform increases with the degree of stupor it induces; the apparent irregularities in the action of the anæsthetic, mainly depending on the varying strength of the vapor employed, on the quality of the chloroform and the constitution of the patient. In order that it may be administered with comparative safety, it is necessary that the proportion of vapor should not exceed three and a-half per cent.; that its effects should be carefully watched, and the inhalation suspended when the required anæsthesia is induced.' The Committee recommend and concur in the opinion generally that ether is an inconvenient anæsthetic, and should be discontinued; the reason for their conclusions is extensively given and can be seen by examining their report. 'A mixture of ether and chloroform is as effective as pure chloroform, and a safer agent when deep and prolonged anæsthesiæ is to be induced; though slow in its action, it is sufficiently rapid in its operation to be convenient for general use. A mixture composed of three parts of ether, two parts of chloroform, and one part of alcohol (by measure), is to be preferred on account of uniform blending of the ether and chloroform when combined with alcohol, and the equable escape of the constituents in vapor; and the Committee suggest that it should be more extensively tried than it has been hitherto in this country.'

"*Chloroform in natural labor.*—The careful administration of chloroform during labor is not attended with special danger, there being, either in this country or abroad, so far as is known to this Committee, no well authenticated instance of sudden death where it has been given by a medical practitioner; but the occasional occurrence of unfavorable symptoms demands the exercise of caution during its employment. Administered in a moderate degree, it does not, as a rule, weaken the expulsive powers, and is decidedly beneficial in promoting the dilatation of the maternal passages. It does not predispose to puerperal convulsions, or other like complications. The balance of opinion is nearly equal as to whether it predisposes to imperfect contraction of the uterus after delivery. As a rule, it in no way retards the convalescence of the mother; nor has it any tendency to interfere injuriously with the function of lactation; nor has it any injurious influence on the child.

"*In abnormal labor.*—The anæsthetic may be employed with advantage in various obstetrical operations,—as forceps, turning, craniotomy, and extraction of the placenta,—unless the patient is much enfeebled by haemorrhage; when, if given, it ought to be accompanied by the use of stimulants. It may also be employed advantageously to check the paroxysms in puerperal convulsions.

"There is no preference in favor of ether over chloroform, unless the latter should disagree with the patient, and in this the practitioner must be the judge. It is very desirable to observe the following rules, during its administration in labor, subject to the discretion of the physician. '*In natural labor*, begin to give

it generally at or after the termination of the first stage; but it may be given earlier if the first stage is unduly painful, or if the os uteri resists dilatation. Give it only during the pains and withdraw it during the intervals. When the foetal head bears on the perineum, give it more freely to promote relaxation and relieve the increased pain. Withdraw the chloroform immediately after the child is expelled. If the patient is depressed or the pains are sluggish during its administration, an occasional stimulant may be administered. In cases where it seems to interfere with the progress of the labor, it may be necessary to suspend its use for a time and reapply it after an interval, or even to withdraw it altogether. *In turning and instrumental* deliveries, deep anaesthesia must be induced, as in surgical operations, and the administration should then be entrusted to a competent person, whose sole duty should be to attend to it."

We thus present our own views, and also those of the Committee, which we hope will be satisfactory to all reflecting minds. The report of the Committee touching the surgical part has not been referred to by us, from the fact, that there is no dispute in regard to the use of chloroform in all important surgical operations. We hold that the obstetrician who can sit quietly by the bedside and witness the appalling suffering of woman in bringing forth her offspring, has a more callous sense of feeling than we should be in possession of at this enlightened period of medical knowledge. We would candidly ask, what is the duty of the physician. It is to assist nature in curing disease, and to mitigate, as far as lies in our power, the suffering incident to disease. The physician who neglects to employ all known remedies to relieve human suffering, deserves the severest censure of the public at large, and especially that of his professional brethren.

Dr. Watters remarked, in reference to the paper of Dr. Johnston, that there was one recommendation in it which had attracted his attention, and on which he intended to make some remarks, viz., that anæsthetic agents should be used in certain obstetrical cases, such as forceps, turning, etc., unless the patient was enfeebled by hæmorrhage, "*when it should be given accompanied by the use of stimulants.*"

The use of stimulants in the treatment of disease had recently attracted much attention, both in Europe and America, and for the last few years he himself had practically studied the use of stimulants and anæsthetics to much extent; and when he said

practically, he did not wish to be understood as studying the subject independently of theory, as the combination was essential in order to arrive at true results. To investigate the subject with any prospect of arriving at correct conclusions and with any hope of acquiring useful facts for practice, it is necessary to inquire the modus operandi of anæsthetics in producing a loss of sensation; and, in order to know when and how to use stimulants, it is necessary we should understand the modus operandi of their effect. In works on Materia Medica, stimulants are divided under various heads, such as nervous stimulants,—nervo-moter,—stimulants to the kidneys, skin, mucus membrane of the lungs, etc., there is left, however, unexplained the important question, *how* are they stimulants? what is that which is common to all, and by which their action is produced? To arrive at a solution of this question we must go back to physiology, as a basis to our inquiry, for all questions as to the modus operandi of medicine, as well as all pathology should find their starting point in the science of physiology.

The usual conception of stimulants is, that they are agents which produce increased action in some or all organs—*i. e.*, they are excitors of action. In all our standard works which treat of stimulants, we find that the increased action is based upon a vague idea of irritability. Irritability is an indefinite term liable to mislead, and as long as pathology is based on such expressions, no true advance can be made in medicine. He had before stated a proposition in regard to the phenomena of life, which he would not now repeat. There was a proposition, however, which he desired to place prominently before the attention of the members, and that was, that every action of the human organism,—whether the movement of a muscle, the process of absorption, of digestion, of assimulation, of secretion,—was reciprocal with an opposite action. This same general law obtains in all physical phenomena, and is as wide spread as the universe itself. All phenomena are connected with an opposite action, motion produces its effects, when resistance is offered, light becomes light through darkness; darkness and light commingled gives vision.

Without the concurrence of this law of opposites, a bird could not fly, or a stone fall to the ground. This same law is also the law of life. He presented this truth with more confidence now than formerly, as all physicists now recognize the principle called the correlation of forces. He did not entirely endorse the theory of the correlation of forces as usually promulgated; only thus far did he recognise it, viz., that action and reaction were equal and opposite, and there could be no phenomena without their combination. Thus, in accordance with this law, the ordinary conditions of life produce destructive change, which is antagonistic to the phenomena of life. These phenomena are just as much in accordance with the law given as physical phenomena. Thus, with the law of action and reaction, the conditions of life tend to produce destruction, and life is reciprocal with this downward tendency. The source of every action in the body being this destructive change, stimulants produce a manifestation of more power, because they occasion the actions which are antecedent. A stimulant is a stimulant only in that it is the occasion of increased waste. The theory is logically consistent; for as destruction is the source of vital power, that which will increase this power must do so by an increase of the destructive process or waste.

There are two classes of conditions necessary to produce the phenonoma of life, viz.: the material which is combustible, and oxygen. The combination of the two occasions the development of power; consequently, we may have a stimulant either by increasing the destructive process by furnishing a supply of combustible material, or the action may be increased by supplying an increased amount of oxygen. The one furnishes the combustible to be destroyed, the other the material by which combustion is occasioned. It follows, then, that when we attend a patient who is depressed, and to whom we desire to administer a stimulant, it is a delicate and important question to decide, whether the depression is due to a want of the combustible, or the oxidizing material, and on this decision probably depends the life of the patient. Alcohol though very combustible, can put out a fire if the supply of oxygen is limited, as oil in quantity will extinguish a

lamp. So a "stimulant" may act by destroying the life of an individual, although he is depressed and needs stimulation; but the little used, far from being a stimulant, is a sedative, and adds to the disease, and consequently to the fatal result. Drs. Todd and Bennet had brought the subject of stimulation in disease prominently and forcibly before the profession, and shown, in many cases where the antiphlogistic plan of treatment was erroneous, and where stimulation could be more advantageously employed. Dr. W. agreed in the main with their opinions, but desired to show more particularly when stimulants should and when they should not be administered. For instance, when a patient is depressed, his condition enfeebled, not for want of material in the body from which waste could be produced and an increased power obtained, but from a want of oxygen, then oxygen should be given and not a combustible.

In regard to the mode of action of chloroform, ether, and alcohol, he proceeded to say, that anæsthetic agents are such in that they consume the oxygen circulating in the blood, and thus deprive the blood of the amount sufficient to produce oxydation of the nervous system corresponding with sensibility. This oxydation is accompanied by waste of tissue, and if this waste cease from any cause, the action also ceases, for action or power is dependent on waste. Anæsthetics use up the oxygen engaged in this process, and do not leave sufficient to act on the sensorium so that the phenonoma of sensibility shall occur. If this position was true he could not see what object would be gained by giving alcohol and an anæsthetic together, when the object was to produce insensibility. If the stimulant acts so as to produce an increased destruction of tissue, more of the anæsthetic would be required under the circumstances to overcome this action, and thus produce any of its peculiar effects. If the administration of the stimulant is intended to benefit the patient, by lessening the dangers of chloroform by stimulation, from the principle which he had attempted to demonstrate, it would become a source of increased danger. Alcohol ceases to be a stimulant when anæsthesia is produced; its action, moreover, is slow,—much more

than chloroform,—and thus after the anæsthetic effect has been produced, the alcohol may seize upon the limited supply of oxygen circulating through the system, thus increasing the danger to the patient. It was the custom, followed by very many persons, when a patient was debilitated from loss of blood by wounds, to administer alcohol abundantly; but he begged leave to differ from the opinion of its usefulness. He thought a cup of tea or coffee was far preferable. In shock, also, he would not give alcohol, although he would give it to prevent shock. When it is the object to sustain the powers of life in disease, when the stimulant is so directed by the organism as to support life, then its use was correct. If the stimulant acts not by being a substance of combustion, but an agent to increase the destructive change of the body by oxidation, then its use would be injurious. Only when the increased action produces some useful object in view, will stimulants, in this way, act advantageously.

Dr. Boislinere said, that he thought chloroform should not be used during the process of the application of the forceps; as, by the partial anæsthesia induced, we lose a valuable guide in the sensation of the patient. The forceps may pinch and we be unaware of it. He used chloroform when the pains were excessive, but not when they were weak. When the patient became excited and exhibited great nervousness, tossing from side to side and suffering from great anxiety, when the pelvis was small, or when the head was large, he used chloroform; not to complete anæsthesia, but only sufficient to quiet the nervous system. When the pains were moderate and the labor proceeding quietly, he did not use the anæsthetic, as it was not then necessary. The great objection he had to its use was, that when once you began its administration you must continue it until labor is over; the patient becomes clamorous for it during her suffering, and sometimes its use must be continued for hours. He never gave it before full dilatation of the os, as the process of dilatation is sometimes very much protracted. The proper time for its administration was, when the sufferings were the greatest, and when the time before complete delivery was short. In certain

cases, where the waters had passed away, and the uterus was firmly contracted around its contents, turning was sometimes necessary, and in such cases was a very difficult operation. Here chloroform might be given; but its use was dangerous, as it is necessary to give so much, because complete anæsthesia must be produced. In some cases, even after its use, the hand could not be introduced. He was rather more inclined to use, in such cases, the plan of Dervees, *i. e.*, to place the patient on her feet and bleed to syncope, when relaxation would take place and turning could then be performed. He thought this plan the safest, as so much chloroform must be given as to constitute it a dangerous remedy.

There was, in his opinion, a marked difference in the amount of insensibility necessary to be produced in ordinary surgical operations, and that where the uterus is in the condition just mentioned. In surgical operations the anæsthetic is used to overcome sensations under the control of the cerebro-spinal system of nerves, and usually a moderate amount accomplishes the purpose. But the action of the uterus, like that of the heart, is under the control of the great sympathetic; and the amount of chloroform sufficient to produce an anæsthesia of this important nerve—sufficient to overcome the contractions existing in the uterus,—would also be very likely to affect the nerves distributed to the heart, and thus to produce death. It was in this way, and in these cases, that he looked upon chloroform as a dangerous remedy, and was disposed to use other and less dangerous methods.

Dr. Hammer thought it was unwise of the naturalist, and would turn out to his disadvantage, to start out in any research with a certain undeniable law; because a man, believing in the infallibility of his law, will make all facts subservient to his doctrine. Dr. Watters had advanced, as the basis of his views, a certain absolute truth, the reciprocality of force; and he thought that the conclusions from such a base could be neither more nor less than failures, as we have not yet discovered absolute truth. Dr. H. viewed the remarks of Dr. W. as a well wrought theory, but he thought he was not fortunate in applying his ab-

solute truth to practice. He admitted that some of the views agreed with his own, but he, Dr. H., had acquired them by looking to facts, not to abstracts. Stimulants had been divided into two classes, one a combustible, the other an oxydizing agent, and the method of action of each described; but only agents of one class were mentioned, viz., the hydrocarbons. In treating of this class, he (Dr. W.) had dissented from the views of Drs. Todd and Bennett, viewing the introduction of hydro-carbon into the system as but adding fuel to the fire, destroying the oxygen of the blood and producing death. This might be true if Todd and Bennett had advised the administration of large quantities of alcohol, but they were too sensible to recommend such a course; they endorsed its use only in moderation, and according to the circumstances surrounding the patient.

Take any severe disease accompanied with fever, and what do we find to be the effect of the fever? Increase of the temperature of the body—not merely an increase of pulse, but an increase of general temperature; and according to the height of the temperature do we have a high or bad fever. The second characteristic of fever is that there is an increased waste of tissue in the organism. Waste of material, then, is the prominent result of fever. If such a fever continues a certain length of time, the inevitable consequence is, that all the material in the body will be destroyed If a muscle is of a certain weight before fever, it will decrease when the fever has continued a certain time. Those laboring under such a fever to whom is administered a hydrocarbon, something is substituted upon which the oxygen of the body can act, and thus the waste of the organs and muscles may be avoided. Elimination of the materials formed will go on, and the waste of the various organs is carried on to such a degree as not to lead to atrophy, as would result were no hydrocarbons given. If the fever took place in a strong man, such medicines need not be given at once, nor for some days; when the system began to feel the effect of the waste, then they might be given. A general rule might be stated, that in order to obviate the too great waste of the body hydrocarbons are almost indispensable.

In regard to the mode of the action of chloroform, as stated by Dr. W., he would only say, that it was an hypothesis. Carbonic acid we know is a gas poisonous in all the tissues. Now if so much of a hydrocarbon be used as to overfeed the body, as a matter of course almost all of the free circulating oxygen, and that within the organs, will act on the carbon of the compound and form carbonic acid. The blood becomes overcharged by it, the lungs will not eleminate it, and its too great accumulation brings on depression of the nervous system, so as to make it insensible. This he thought an easier method of explaining the action of hydro-carbons and the production of insensibility.

As to the use of bleeding in cases of a firmly contracted uterus, he remarked, that if the effect of large bleedings were better understood, they would not be employed. Rational medicine in our day has proved by experiment what bad effects result from any great abstraction of the vital fluid. Dr. B. had remarked that bleeding would produce dilatation, and again, that in some cases, after complete anæsthesia, the hand could not be introduced. He was compelled to differ with these statements. Venesection did not produce such relaxation as to enable one to turn easily. He recalled one case which had occured to him years ago, where the patient had been in labor for three days. The waters had been discharged, and the head presented, but the uterus was closely contracted around the fœtus, and the os was almost as hard as stone. He feared rupture of the uterus. He brought the patient under the full influence of ether, and found to his satisfaction that relaxation took place, and he was enabled to introduce his hand and turn. He could quote other similar cases. He thought there was no remedy which would produce such relaxation of tissue as chloroform. In incarcerated hernias many remedies had been used with greater or less advantage, but all were now superceded by chloroform as an aid to taxis. No surgeon now would use any other remedy. The uterus, he thought, was no exception to the rule.

Dr. Newman recalled a case illustrative of the good effects of bleeding. He was called in consultation some years ago. The

woman had been in labor 36 hours, the arm presented, and the uterus was so firmly contracted around the fœtus that the finger could not be introduced through the os. She had been nauseated without effect. Bleeding was resorted to. The patient was propped up in bed, and a vein opened by a large orifice, and the moment syncope was produced, his hand easily glided through the os, turning was effected, and the patient safely delivered. In St. Louis he had seen a similar case, where chloroform and every thing else had been used except bleeding, and the patient died. If another case came under his charge, he would unhesitatingly use bleeding.

[To be continued.]

Review of Current Medical Literature.

GENERAL PATHOLOGY AND PATHOL. ANATOMY.

1.—*Clinical Researches on Fever*, by Dr. C. Liebermeister, Prof. in Tübingen. (*Prager Vierteljahrschrift*, 1865, Vol. 1 and 3.)

A rare accuracy of investigation and depth of reasoning is devoted to the subject of fever in these articles. The laws which the author develops in the first article, though not new in themselves, are supported by new experiments and proof of great precision; they are couched in the following words:

"Elevation of the temperature of the body is the constant and pathognomonic symptom of fever.—During fever an increase takes place in the *production* of heat.—The increased production of heat depends upon an increased metamorphosis of tissue.—Every fever, (that also which accompanies inflammatory processes,) presupposes a *general* increase of metamorphosis."

"*Definition:* Fever is a complex of symptoms, based upon an increased temperature of the body, caused by a pathological general increase of metamorphosis of tissue."

L. considers this definition highly incomplete; but the limits of the certainly demonstrable would have been exceeded by enlarging upon the cause of the acceleration of the metamorphosis; yet he seems to lean toward the doctrine of a functional disturbance of parts of the nervous centres, as indicated by Wunderlich and Virchow.

The second article treats of the changes of weight of the body in chronic diseases, and its relations to fever.

The danger of fever consists in the consumption of tissue it causes, and in the effect of the elevated temperature. Leaving out of consideration those exceptional cases, in which the appetite and digestion are normal or even increased, so that it becomes possible to introduce a supply of material corresponding to the increased waste, every fever must lead to a diminution in the quantity of the tissues. Especially in acute febrile diseases does the diminution of weight of the body become evident, but it is generally, also, easily recognized in chronic febrile affections. Some years ago, Hutchinson (*Med. Times, Aug.* 1850,) has furnished numerical proof for the fact, that in pulmonary phthisis the changes in the weight of the body are often more essential to diagnosis and therapeutics, than all other symptoms. But diminution of weight may be due to other causes besides fever, and in order to make practical use of it as a symptom, it must be traced to its special cause in each individual case. These causes are chiefly local diseases in organs subservient to the reception or assimilation of nutriment, and excessive losses by secretion, etc., (*Sæfteverluste*). The effect of the latter, however, has usually been overestimated as compared with other causes of consumption. These considerations acquire especial importance in the question of the rapid and excessive emaciation in pulmonary phthisis, where we speak of "hectic" fever, but rather with attention to the accompanying diarrhœa and excessive sweats, than to the effects of the fever *per se*. The loss by expectoration in phthisis is really unimportant; bronchiectasis often causes a four-fold greater actual loss without emaciation. The impairment of respiration also may exist without emaciation, *so long as*

there is no fever. Of much greater effect is the disturbance of the appetite, the reduced supply of food; but still, in other diseases, where fever is absent, this element operates much more slowly in causing emaciation, than might be expected. On the other hand, cases of miliary tuberculosis occur, in which the fever leads to rapid consumption, before any destructions in the lung or essential lesions of functions have taken place. The great, and often decisive, effect of the fever as the source of consumption becomes particularly evident, when we compare accurately the state of the temperature and the weight of the body in a greater number of cases. Instances of advanced phthisis are not rare, in which, without an improvement in the local affection, the consumption ceases to progress or the weight of the body increases, even, as soon as the fever abates.

These, and many related facts (which the author has fully illustrated and supported by clinical histories and experiments,) compel us to ascribe to the fever a greater importance as a *proximate* cause of the bodily waste in most cases of febrile diseases, than to the local affection. [Looking beyond the immediate causes, however, the local affection undoubtedly takes higher rank in the mediate causation of consumption, for there is no doubt but that the fever, aside from the so-called essential fevers (acute infections), depends chiefly upon the intensity and extent of the local disease.]

In judging of deviations of the weight of the body, another circumstance requires consideration: "there is in every individual a tendency to the preservation or recovery of the normal weight, and this tendency is, *cæteris paribus*, the more effective the farther the weight had sunk below the normal." (Thus we frequently see a patient, much reduced by an acute febrile disease, when entering upon reconvalescence, suffer a remarkable increase of weight, although the supply of food does not exceed the normal.) This tendency to the restoration of the normal weight consists in a dependence of the intensity of metamorphosis upon the relative weight, *i. e.*, the momentary state of nutrition of the individual. There is proof that, in fever, the decay of

nitrogenous compounds is not always increased *absolutely*, but often falls below the normal standard. Hence, the law that fever consists in an abnormal increase of general metamorphosis must be interpreted to mean a *relative* increase. In some fever-patients the daily waste of substance is less than it had been in the same individual during health; but still greater than it would be under entirely similar circumstances in the absence of fever. For this reason, also, we can regard as pathognomonic of fever neither the increase of consumption nor the increase of the production of heat, but only the resulting elevation of temperature, which takes place in all cases of fever, without exception

After solving the apparent paradox of a want of correspondence between the production of heat and the metamorphosis of matter, and referring to febrile consumption as the probable cause of marasmoid conditions, the author concludes:

"The most important result of the above considerations, to which I shall have to refer frequently in subsequent articles, is the proposition that every fever produces a relative increase in the waste of the substances of the body. Since in most cases this increase of waste cannot be balanced by a corresponding increase of repair, a diminution of the tissues results, and after a longer duration an oligaemia of high degree and general atrophy arise, which either gradually lead to death by themselves in a more or less direct manner, or else cause other influences to prove fatal, which in a less enfeebled body would not be dangerous. *In the febrile consumption, therefore, lies one of the most essential dangers of fever*, and the consideration of it is of the greatest importance in proposing therapeutical indications."

2.—*On the Pathological Appearances presented in Marsh Fever.* By J. Forsyth Meigs, M. D. (*Amer. Journal Med. Sc., Oct.* 1865.)

Having given the history of four cases of malarial disease, which illustrate the peculiar occurrence of pigment in the blood, the author proceeds to quote the discoveries of Prof. Frerichs, as detailed in his work on Diseases of the Liver. After describing pigment-liver, and the melansæmic changes in the spleen, brain, kidneys, etc., he continues:

"The pigment matter is found in abundance in the blood, and particularly in that of the portal vein, and is thence conveyed to the different tissues and organs in which it is discovered. The usual form is that of small, rounded, or angular

granules, which are sometimes isolated, or more frequently connected together in groups by a pale substance, soluble in acetic acid and caustic alkalies. True pigment cells are observed along with the granules and granular masses, though in somewhat smaller quantity. They resemble in size and form the colorless corpuscles of the blood, or they consist of large spindle or club-shaped cells and rounded nuclei, with sharply defined walls. These cells contain a greater or smaller number of black granules. The color of the pigment is usually deep black, more rarely brown or ochre-colored, and, less frequently of all, reddish yellow.

"It is thought by most observers that this pigment matter is formed in the spleen. Dr. Frerichs is of opinion that, though there are many reasons for such a belief, there is no proof that it may not be formed in other portions of the vascular system. He believes, however, that there is no doubt the larger portion of the pigment is formed in the spleen.

"In malarial disease, particularly, there is every reason to suppose that the spleen is the principal seat of formation of the pigment. During the congestions of that organ, which so constantly occur in this disease, the stagnation of the blood in the venous sinuses gives rise to changes which result in the formation of pigment in the masses of stagnant blood. Frerichs supposes that the club and spindle-shaped pigment cells are the epithelium of the lining membrane of the sinuses infiltrated with the decomposed red matter of the blood, that the globular pigment cells are colorless blood corpuscles infiltrated with molecules of coloring matter, and that the pigment scales are the broken-up fragments of the coagulum.

"The results of these changes of the blood in the spleen are admirably drawn. One of the first effects is the production of chlorosis or anæmia, by the destruction of the red corpuscles, and no one who has seen the rapid advance of pallor in a case of unchecked or obstinate malarial disease, can fail to be pleased with so admirable an exposition of the mode of production of the phenomenon. The pigment, carried by the spleen to the portal vein, and thence to the liver, is, in part, arrested in the capillaries of that organ, whilst other portions, consisting of the smaller granules and cells, pass on through the liver to the general circulation, and so reach the lungs, brain, and kidneys.

"It is this retention of the pigment matter in the capillaries of the different organs by which Dr. Frerichs explains many of the symptoms which accompany certain of the severer forms of the disease. Thus, the retention of the coarser fragments in the capillaries of the liver, by clogging those vessels, gives rise to stasis of blood in the venous radicles of the portal vein. This stasis explains the intestinal hemorrhages and the diarrhœas which so frequently attend the more violent cases of bilious fever. In other instances, in addition to the effusions taking place from the mucous membrane of the alimentary canal, we have serous effusion into the peritoneal sac, occasioning ascites, a result similar in cause and symptoms to the ascites caused by cirrhosis.

"The cerebral phenomena, the stupor, delirium, convulsions, or paralysis, which sometimes occur in miasmatic fever cannot, according to our author, be so satisfactorily shown to depend on the retention of pigment in the capillaries of the brain, as the intestinal hemorrhages, diarrhœa, and ascites, have been

shown to be the result of hepatic obstructions, but that a connection between the two does really exist, at least in some degree, can scarcely be doubted.

"In many severe cases, it was found that the albuminuria and general dropsy present during life, evidently resulted from the obstruction to the renal circulation occasioned by the retention of pigment in the capillaries of the kidneys, and particularly in the Malpighian bodies.

"Frerichs states that, 'considering the great frequency of intermittent fevers, the cases in which there is a marked development of pigment are particularly rare; hence, in such cases other agencies, of which we possess no accurate information, must co-operate with the usual causes of intermittent fever. In the present defective state of our knowledge as to the nature of infectious diseases, it cannot be determined whether a particular quality, or an unusual intensity of miasm, is necessary for the purpose.' The cases which he has observed and published, occurred after an inundation in Silesia, resulting from an overflow of the Oder, in 1854. Since that period cases of this kind have been very seldom observed, although the ordinary forms of intermittent fever are never absent. He remarks that a perfectly accurate diagnosis can only be made by direct examination of the blood. A few drops carefully collected are sufficient to show the presence or absence of large quantities of pigment. He usually collects it by means of a cupping-glass, care being taken to prevent the admixture of foreign matter.

"The four cases given above are all well-marked examples of the disease, the pathological results of which Dr. Frerichs describes so well. The first case is one of simple intermittent, arising in one of the higher latitudes subject to marsh miasm, a form easily overcome by proper treatment in the early stage, and yet, even in this case, owing to its being allowed to run on unchecked for some weeks, we found in the blood of the patient, taken from the finger, the peculiar pigment granules described by Frerichs.

"The other three cases were of a very different type. Originating in one of the lower latitudes, they exhibit a severity which gives us the opportunity of observing the serious consequences resulting from a dose of marsh miasm of the most poisonous kind. One of the three, though very ill when he reached the hospital, was saved by proper treatment. One was removed from the hospital in a condition which precluded all reasonable hope of his recovery. The third died. This case presented all the severest effects which result from the action of the malarial poison on the human economy; periodical fever, rapid loss of strength and flesh, with sudden production of anæmia, and, somewhat later, diarrhœa, ascites, anasarca in connection with albuminous urine, general cachexia, and death. With Dr. Frerichs's work before us it is easy to understand the successive steps of these changes. The congestion and stagnation of blood in the spleen explain, by the actual destruction of the red globules, the speedy induction of anæmia. This condition is also greatly favored, in all probability, by the failure of the globule-making function of the gland. As the destruction of the globules proceeds, their red matter is converted into pigment, which pigment, in its various forms, is carried into the vascular apparatus of the body. Deposited in the liver it may accumulate to such an extent finally as to impede

the portal circulation and give rise to diarrhœa and ascites. Lodged in the vessels of the kidneys, and specially in the Malpighian bodies, it may cause a like impediment in those organs, and there is developed a true albuminuria with its consequent anasarca. In the third case given above we had all these conditions. Pigment in the liver, with diarrhœa and ascites. Pigment in the kidneys, with albuminous urine and anasarca.

"The lessons as to treatment to be drawn from these discoveries are simple. The beneficent action of cinchona can scarcely be raised higher than before, but its precise mode of action is more clearly developed. Its anti-periodic power is that which makes it invaluable. By cutting off the paroxysms, it prevents the repeated congestions and stagnations of blood in the spleen, and thus arrests the destruction of the blood in that organ, and the consequent production of pigment, which is the obstructive agent carried into the more distant organs of the body. The one most valuable therapeutic law taught by these facts is the necessity of applying the saving agent early in the case. When so used it averts the disastrous effects of repeated paroxysms, and thus saves life as clearly and distinctly as does a surgical operation in a case where no other course of procedure is possible. Reason deduces this result as plainly in the former case as the eye beholds it in the latter."

3.—*Cancer of Lymphatics (of the Pleura and Lungs).* By Dr. E. WAGNER. (*Schmidt's Jahrb.*, cxxvi, p. 159, from *Archiv d. Heilkunde.*)

"Cancer of lymphatics is usually considered only a secondary filling of the lymphatics with the juice of cancer, which was derived from a peripheric cancer and was transported farther in them. (Andral, Guensburg, Cruveilhier, Rokitansky *et al.*) The author observed in four cases that cancer sometimes arises in the walls of lymphatics and proliferates into their interior. Even the microscopic appearance showed, that the cancer was seated in the lymphathics. The pleura contained a comparatively close net-work of variously thick, regularly or irregularly varicose stripes, which on cutting into them emptied their contents, upon pressure, in the shape of soft thrombi. The net-work consisted of superficial, sparse, and usually thin canals, as well as of deeper seated, denser and better filled ones. * * * * * * *

"That the cancer-cells had not in some way been introduced into the lymphatics, but had originated in the vessels themselves, was evident, according to the author, from the complete absence of any change in the cells; from the fact, that the lymphatics contained cancer-cells in localities where the peripheric parts were free from cancer; and from the disposition of the cells in the lymphatics and their relation to the walls: for the cells were partly adherent to the inner surface of the lymphatics in the same manner as, e. g. the cylindrical cells to mucous membranes; but the greater part of the cells were attached in aggregations of 20, 30 or more, by peduncles more or less long, to excrescences from the inner surface of the vessels."

4.—*The Pathological Anatomy of Tetanus.*

In a communication to the Royal Medical and Surgical Society of London, J. Lockhart Clarke—

describes the condition of the spinal cord in six cases of tetanus. In every one of these there was not only more or less congestion of the blood vessels, but there were also definite, and frequently extensive, lesions of structure, such as have never yet been discovered. These lesions consisted of disintegrations of tissue in different stages of progress, from a state of mere softening to that of perfect fluidity, and were accompanied by certain exudations and extensive effusions of blood. They were found chiefly in the grey substance, which, moreover, was in many places strangely altered in shape—unsymmetrical on the opposite sides, or partially fused with the adjacent white column in a common softened mass. Although lesions of this kind existed, in one form or other, in every *region* of the cord, they were absent in some places; nor did they ever for long together maintain the same shape, size or appearance, but were constantly and alternately increasing, diminishing, or disappearing, at short but various intervals.

These lesions in tetanus are precisely similar in character to those which the author has discovered in the spinal cords of many ordinary cases of paralysis; and on comparing together the lesions and symptoms of both diseases, he finds good ground for the support of the following conclusions:

First.—That the lesions are either not present, or are present only in a slight degree, in those cases of tetanus which recover.

Second.—That they are not the *effects* of the *great functional activity of the cord*, manifested in the violent spasms, but are the effects of a morbid state of the blood vessels.

Third.—That they are not alone the *causes* of the tetanic spasms.

Fourth.—That the tetanic spasms depend on *two separate* causes—firstly, on a *morbidly excitable condition of the grey substance of the cord*, induced by the hyperæmic and morbid state of its blood vessels, propagated from the injured nerves, and resulting in exudations and disintegrations of tissue; and secondly, on *irritation* propagated and spread through the morbidly excitable cord from the same source—from the periphery, by the diseased nerves.—[*Lancet, Oct.*, 1865, *p.* 494.]

PRACTICE OF MEDICINE.

1.—*On the Auscultory Signs which precede Crepitation in Pneumonia.* By A. T. H. Waters, M. D. (*Lancet*, April, 1865, p. 185.)

"It is very rarely that an opportunity is afforded us of making an examination of the chest in incipient pneumonia, and to this we must attribute the differences of opinion which have been expressed as to the earliest physical signs of the disease.

"Most authors, since the time of Laennec, have regarded the crepitating râle as the first recognizable sign in pneumoniac inflammation. Dr. Stokes has, however, asserted that an 'intense puerility of respiration' precedes this phenomenon. 'The physical signs of the first stage of pneumonia,' he says, 'are still to be determined with accuracy. Without possessing a sufficent number of observations to determine the point, I am led to the belief that an *intense puerility of respiration in the affected part* will be found to be the principal phenomenon. In cases in which inflammation was spreading upwards, I have often found that a puerile respiration preceded the crepitating râle for some hours; and that this was not a general, but a partial, condition, was shown by its being much more intense in the vicinity of the disease than in the opposite lung. Indeed, in cases presenting great puerility of respiration with fever, we may often prognosticate the occurrence of the crepitating râle. Thus in a case in which the disease attacked both lungs as well as the pericardium, I observed this sudden appearance of intensely puerile respiration on three distinct occasions — in two, it was followed by the crepitating râle and other signs of pneumonia, and in one it was removed by bleeding before the above signs had occurred.'

"I think objection might be fairly taken to Dr. Stokes' conclusion, founded on his observation, that a puerile respiration is heard in a pneumonic lung above the level of existing crepitation; for it might be said that near to the inflamed portion the lung would be doing greatly increased work, and thus we should expect to meet with a loudness in the respiratory murmur. Nor would such objection be fully met by his statement of the fact that the murmur was less puerile in the opposite lung than above the seat of crepitation. But the observation on which Dr. Stokes mainly depends in support of his view, is the one he last refers to. In the case in question, he met with the phenomenon on three distinct occasions, and in two of these unmistakable evidence of pneumonic inflammation followed.

"To render the observation of this phenomenon, however, perfectly trustworthy, it must be made on a case not where there is progressive inflammation, nor even where there are consecutive attacks of inflammation, but where, the lungs having been previously healthy, acute primary pneumonia comes on suddenly. Two such cases have recently occurred, under my care, and have enabled me to confirm, by observation, the view which was originally advanced by Dr. Stokes. * * * * * * * *

"M. Grisolle, in the recent edition of his work on Pneumonia, referring to the alleged puerility of the respiration in the disease, says that he has never met with it as an initial symptom, or as a precursor of the crepitating râle. He has, however, he says, frequently called attention to another phenomenon which he has observed in some cases of the disease, viz: a weakness of the respiratory murmur. He has observed this, he says, on several occasions as a precursor of crepitation.

"I have never met with the physical sign which M. Grisolle speaks of; but of the existence of a loud, harsh respiratory murmur, in the cases I have referred to, as an initial symptom of pneumonic inflammation, there can be no doubt. And although I would refrain from asserting that in all cases of pneumonia this

harsh respiration precedes the crepitating râle, yet I cannot but think that the state of the respiratory membrane, at the commencement of the affection, a subject to which I shall refer in a future paper on the morbid anatomy of the disease, is just that which would be likely to produce the phenomenon."

2—*On Dyspepsia with increased secretion of Mucus.* By GUIPON, in *Bull. de Ther.*, lxvii, p. 320. (*Schmidt's Jahrb.*, CXXV. p. 301.)

"The characteristic of this disease is the production and vomiting of clear, albuminous, mucous or watery fluid, which is secreted independently of digestion before or after meals. Without other disturbances of digestion the patient is seized with nausea, retching and vomiting of fluid matter; food is seldom vomited at the same time. Usually the vomiting takes place easily—sometimes the act of rising from the bed, or an ordinary cough, suffices to expel, the more or less considerable quantity of fluid collected from the stomach during sleep; rarely it is accomplished only after violent and painful efforts. This mucous vomiting occurs once or several times a day; in the former case, a sensation of fulness and pains in the epigastric regions set in, especially if the discharge in the morning was incomplete.

"The affection is by no means so rare, nor does the use of alcohol form a predisposition to it. The author is inclined to consider the affection as a neurosis with hypersecretion, the cause of which is to be sought less in the walls of the stomach themselves than in the plexus and other nervous elements. Dyspepsia, with increased mucous secretion, is very often hereditary, and sometimes occurs with all members of the same family. Aside from hereditability, frequently repeated irritation of the stomach by stimulants is followed by profuse secretion of mucus. In some localities this form of Dyspepsia seems to be endemic; it then often disappears upon the occurrence of a neurosis in another more or less distant organ, and is not, therefore, the precursor of an organic affection of the stomach, as some assert.

"In the acute, and not deeply rooted, chronic forms, the affection easily yields to the use of a purgative and light, dry diet. The old chronic forms seldom or never can be completely cured; but the most annoying symptoms can be easily alleviated. The author is accustomed to prescribe in such cases, with excellent success, a strong dose of powder composed of Potass. tartr., Magnesia and Jalap; repeated, if necessary. He recommends, besides, preparations of iron, the waters of Vichy, and sulphur springs."

3.—*On the indications for the use of Calomel in the treatment of Dysentery.*

The *Gazette Hebdomadaire*, (2 *June*, 1865,) quotes the conclusions at which Pécholier arrives, in an elaborate article in the *Montpellier Medical* on this subject. The character of dysentery varies according to climate, season, epidemic influence.

There are numerous cases, especially in warm countries and seasons, which demand above all, and sometimes exclusively, the use of cathartics. Among these, some are accompanied with a degree of erethism and gastro-intestinal irritation, which prevents the administation of most purgatives, for a time at least. This variety of dysentery was very conspicuous in the summer of 1864, at Montpellier. While the yellow complexion, furred tongue, anorexia, nausea, the pains in the right hypochondriac region, and the general good effect of cathartics in the reigning character of disease, invited the use of purgatives,—the fever, the redness of the margin of the tongue, the acuteness of abdominal pains, and the quantity of the stools, which consisted of nearly pure blood, made us fear to augment, by a purgative, the intestinal irritation. Knowing the double action, purgative and sedative, of calomel, the author thought this remedy appropriate in this species of dysentery. Usually after a slight increase of the symptoms, a diminution of the colic and tenesmus and of the frequency of the stools took place; the latter became those of diarrhœa, and promptly ceased; appetite and proper action of the intestines returned; fever and thirst abated,—in short a prompt and lasting convalescence took place. A light participation of the liver yielded promptly to this treatment. The only inconvenience was the stomatitis induced, which, however, was more than balanced by the revulsion effected in the congestion of the intestinal tube.

Other medication has been combined, according to circumstances, with the calomel; an antiphlogistic treatment in excessive erethism and intestinal irritation; opium from the very beginning, when pain predominated. Whenever, therefore, an acute dysentery is accompanied with much excitement of the circulation, or of the nervous system, and with much irritation of the intestinal canal, so as to contra-indicate emetics and active purgatives, calomel is the appropriate remedy. But the author has seen no marked advantage in the use of calomel in chronic dysentery.

The dose employed was one gramme in six parts, (one every three hours,) repeated on the following day in the same way.

4.—*Differential Diagnosis of Ascites and Hydrovarium.* By Prof. BRESLAU, of Zurich (*Wiener Med. Presse*, 1865, *No.* 1.)

The author remarks that whoever has frequent occasion to diagnose between these two affections, will know that some cases present so much difficulty that errors have been made by the most experienced physicians. Without recapitulating the entire symptomatology of the two conditions, he desires to call attention to a single sign, which, in the great majority of cases, is of decisive value:

"*Whether the patient is affected with ascites or hydrovarium, we can draw certain lines on the abdomen separating the dull sound of percussion from the sonorous, tympanitic sound. But while, in hydrovarium, such a line also limits the fluctuation, in ascites fluctuation always extends farther than the dull percussion sound; it exceeds the latter to an indefinite extent in places where percussion still yields a tympanitic sound.*

"In this proposition, indeed, I have expressed all I desire to say, but I must add a few remarks in explanation.

"In ascites, the tympanitic intestines are kept floating upon the fluid collected below them and playing around them, and percussion demonstrates the level of the fluid around the intestinal coils. Now, as soon as the abdomen is agitated, as is necessarily done in determining fluctuation, some of the fluid is forced between and in front of the floating intestines, and fluctuation is then felt in places, which, immediately before and after, percussion showed to be occupied by intestines. For, even in the largest ascites, the fluid has not occupied all the space in the peritoneal sack; it will yield when put in motion by agitation, and we perceive this yielding when we test for fluctuation in places where the fluid is forced between the intestines.

"It is otherwise in hydrovarium. Here, also, by percussion, we define the intestines against the existing fluid; here, also, fluctuation exists, and in many cases is extremely distinct and superficial, as in ascites. But the limit of fluctuation will always coincide with the limit of the dull sound; fluctuation will not be perceptible where we find intestines by percussion, but will be confined to that region in which the fluid gives a dull sound. Here the fluid is collected in a sac, which it cannot leave; the intestines are separated from the fluid by the walls of he sac, and whatever may be the agitation, the fluid will never pass between or in front of the nearest portions of intestine, and hence will never be perceivable by palpation."

5.—*On the Mountain or Continued Miasmatic Fever of Colorado Territory.*

Dr. Waggoner calls attention (*Amer. Journal Med. Sc., July*, 1865, *p.* 50,) to a continued fever, with which he became

acquainted during a few years' residence in Colorado, in civil and military practice, and which he regards as "purely occidental, and indigenous to Colorado and the Rocky Mountains." His cases occurred mainly in the fall of 1863, after a September full of intermittents and remittents of ordinary type and grade.

"The cases of the continued type all presented an icteroid appearance, dejected countenance, eyes sunken and hollow, epigastric tenderness, urine highly colored and scant. Delirium soon came on, and, as a general thing, in two or three days the 'brave soldier boy' lost all consciousness of his whereabouts.

"The most assiduous observation failed to detect any remission of the fever, either by day or by night. The leading symptoms that now ensued were delirium; constipation, slight but not serious; gastric derangement; first a light, but later a dark furred tongue, though never concave and excoriated as in typhoid cases. Pulse from 120 to 130, usually small and weak; rarely any vomiting—if any, readily controlled; no enteric complication or tympanitis; hot skin, thirst, immoderate and continued burning fever."

The writer details one of three cases treated without quinia, all of which were tedious, persisting for "thirty and thirty-five days before any marked signs of convalescence." After this experience, the author "resolved to push quinia until (he) obtained more marked effects." Two such cases are related; more than two drachmes of quinia were given; each ended in rapid convalescence in a week; they are—

"samples of some twenty cases treated in like manner and with like results. No case yielded with less than sixty grains, and seventy-five was not exceeded in one day; and in no case did any unpleasant symptom follow, but most perfect convalescence.

"It would be extending this paper too far to detail all the cases, and as these two are complete samples of the type of the fever, and my treatment being the same in all, I think them sufficient. During the remainder of the fall I continued this practice, and did not lose a patient, all being promptly relieved by the early administration of quinia. In fact, a large majority never reached the febrile stage, which doubtless would have proved long and tedious had it not been for early treatment; only the two cases run their limits of thirty-five and forty days. As winter came on, this type disappeared, and nearly all the garrison that had escaped fever were affected more or less with jaundice, which affection yielded readily to blue pill and podophyllin. Podophyllin I consider a valuable adjuvant to mercury, the mercury depriving it of its drastic properties. It may be asked, Were the secretions yet dormant when the quinia was administered? I answer, yes. They were affected but little by the mercury first administered. But in this I consider quinia one of the best cholagogues we have, not immediately, but

secondarily. In every case, as soon as the fever was broken up, all the secretions, including that of the liver, at once commenced to flow."

[Although the author considers this *type* of malarial disease "*purely occidental*," with the above definition of "Occident," we are inclined to believe it identical with the cases—frequently seen in this very city, and indeed in all the Western States—of continued fever developed from ordinary malarial forms; but the treatment is rather unusual, and would seem to merit attention.]

6—*On the treatment of Typhus Fever.*

In England a very decided reaction against the treatment of fevers, especially typhus, by alcoholic stimulants, is setting in, initiated by such very able authorities as the editor of the Lancet, Dr. Wilks and Dr. Gairdner, not failing, of course, to call forth an able defence of the stimulating treatment by equally eminent men, as Bennet, Kennedy, *et al.*:

[Dr. Wilks gives us twelve cases of fever which occurred in Guy's Hospital as illustrative of his treatment of typhus and typhoid without stimulants. As the treatment of fevers, pneumonia, and other diseases by large quantities of stimulants has lately become popular with many members of our profession, we advise them to read with attention the following remarks by Dr. Wilks, the Editor of the Lancet, and Dr. Gairdner, of Glasgow.]

With respect to the treatment of fevers (observes Dr. Wilks) I adhere to the old practice of the hospital—that which was laid down by Drs. Bright, Addison, and Barlow, in their lectures, and which they adopted for so many years with success. These teachings of these professors was to the effect that a large number of cases ran their course without any other treatment than careful watching and feeding; that they required no other medicine than a simple saline; but that some needed a stimulant during the progress of the disease, and others required it from the commencement. The question, therefore, with regard to the administration of stimulus, was *when* to give it, and in what quantity. At the present time there are advocates for a universal method in favor of alcohol in all cases of fever, just as there are those who indiscriminately administer ammonia in scarlet fever, and who, when failing to prove its value in all cases, fall back upon the explanation that if alcohol or ammonia be of real service in a bad case of fever or scarlatina, and if these remedies do no harm in the milder forms, it is a good rule to administer them universally. Such a method is not only unscientific, but I believe positively injurious; for in many cases of typhus fever in young people, where the brain has been involved, I have a very strong opinion that the brandy which I have seen given in such cases has been positively hurtful. In my intercourse with medical men I judge that very many are scarcely alive to the fact that typhus fever is very rarely fatal in young persons

—the prognosis, indeed, only becomes serious as years are added to the age of the patient,—and therefore that they are too apt to attribute recovery to their remedies. Young persons always do well if left alone. Of this fact I could now quote a large number of cases in proof, and, on the contrary, the few instances which I have seen end fatally have been those in which a large amount of stimulus was given from the commencement of the disease; and, what perhaps is even more to the point, the withdrawal of stimulus in some cases where it was adopted as the method of treatment, has been attended with the most decided advantage.—[*Braithwaite's Retrospect, July*, 1865, *from London Lancet.*]

Dr. Gairdner gives his experience during "two months of fever duty in the Glasgow Royal Infirmary":

[The cases admitted into the male wards during the period of Dr. Gairdner's attendance were 68 in number; of these, however, 61 only were typhus or typhoid fevers.]

The treatment, where not otherwise specified, has been conducted mainly on the following principles:—To afford a bland and nutritious aliment throughout the fever, chiefly consisting of milk and water, buttermilk, rice and milk, arrow-root, and beef tea, sometimes with a little bread, where it was relished. The choice of the special form of diet was to some extent determined by the feelings of the patient, and to a like extent left to the discretion of the nurses, but milk diet in all instances formed the staple nourishment. Farther, it has been considered expedient to employ no medicine *as matter of routine*, so that where no statement to the contrary is made, it may be presumed that no medicine at all, unless it may have been an ordinary laxative, or a little common cough mixture or the like, was administered throughout the disease. A similar remark applies to the administration of alcoholic stimulants, except that I have thought it right, in view of certain questions recently under discussion, to state distinctly, as from the hospital wine and spirit roll, which is kept with very great accuracy, the whole amount of stimulants given in each case; and where none were given, to make an express statement to that effect.

Dr. Gairdner then describes a long series of the most interesting cases, and concludes a long paper by the following hints on treatment.

The first lesson that has to be learned in dealing with fever, is at the same time to many minds the most difficult—to let well alone. And those who naturally or by education incline to a more or less active routine practice, are not, I believe, likely to be convinced by statistics, or by any form of abstract reasoning, that such practice is either unnecessary or injurious in such a disease as typhus fever. The argument, to many an irresistible one, will always recur—What! are we to stand by and to do nothing at all, when the pulse is 120, the tongue dry, the brain oppressed and delirious? and it may even be admitted that the question is an extremely embarrassing one for all who are not prepared to answer it out of a matured and carefully studied experience. But in view of facts which have become clearer and clearer in proportion as our knowledge of typhus fever has

been defined and rendered more exact, it has become a duty with teachers of medicine, not indeed to acquiesce in a blind expectancy, any more than in any other unreasoning routine, but to take a firm hold of principles based upon experience, and to place these in the light of such practical illustrations as their opportunities, derived from hospital practice, enable them to command.

I would therefore invite the attention of all physicians who admit any doubt as to the proper practice in typhus fever, and especially of all who have still to attain practical experience on a large scale, to the cases here recorded, with the view of showing:—

1. That at least two-thirds, more probably three-fourths, perhaps even a greater proportion, of the cases of typhus fever in the present epidemic, may be successfully treated without either medicine or stimulants; or at least, with only such slight accidental doses of either as cannot be supposed to influence considerably the result of the fever, as such.

2. That in the cases of typhus fever recorded in this paper, the treatment, whether by stimulants, or medicines, or both, has been directed entirely to emergencies and casualties, and not to the subduing of the febrile excitement, or the restraining of the typhoid delirium, or the reduction of the frequency, or increasing the strength of the pulse; on the principle that fever, with rapid weak pulse, and a certain amount of typhoid delirium, are to be regarded as the normal facts of the disease, always observed within certain limits of time, and not to be avoided or cut short, by any known specific treatment; from which it follows, that remedies, especially very active remedies, are to be reserved chiefly for manifest variations from this normal course; as when tartar emetic has been given, in small doses with wine, in numerous cases of bronchitic complication, or wine and whiskey in obvious tendency to collapse; or to take a less extreme instance, a purgative in accidental constipation, an emetic in gastric irritability, lime water in diarrhœa, or an opiate in case of protracted sleeplessness in the early stage.

3. That under a practice so regulated, the mortality of typhus fever has been not more than 9.3 per cent. in the cases here presented, which have been treated absolutely without selection, and without excluding a single fatal case on any ground whatever.

4. That in children and young persons, probably up to the age of twenty, or at least fifteen, typhus fever, so treated, is a disease of hardly any danger.

5. That in all cases where a regular daily note of the pulse shows a gradual climax, ending in a maximum rate on the 8th, 9th, 10th, 11th or 12th days, or even later, and afterwards a gradual decline in the rate, there is no occasion for alarm after the crisis of the pulse has been reached, even although delirium should go on, hardly abated, for several days; and that no sudden crisis is to be expected, or wished for, in such circumstances.

6. That nourishment, of the kinds indicated at the beginning of this paper, is to be carefully administered throughout the course of typhus fever. That free ventilation is also indispensable.

7. That good nursing, and constant watchfulness against casualties; especially watching of the chest, the evacuations, and the skin of the back, are the real

"physic" needed by all fever patients; and more particularly by the old, the intemperate, and the subjects of chronic disease, in whom the danger is incomparably greater than in the young, healthy and temperate.—[*Braithwaite's Retrospect, July*, 1865, *from London Lancet.*]

In the Imp. Society of Physicians in Vienna, Dr. Flamm spoke on the employment of opium in typhus, without symptomatic indication. (*Wiener Med. Presse*, 1865, *p.* 582.)

"The treatment of typhus which is now usual, is a symptomatic one, into which, among other remedies, opium has been introduced more and more, but in general only against certain symptoms, as catarrhs of the respiratory passages, diarrhœas, intestinal hemorrhages, etc. The speaker found, that the beneficial influence which opium exerts in the course of typhus must not be ascribed solely to its combating these symptoms, but most particularly to the *sleep which it produces artificially at the same time*, and that typhus patients generally, in the early stages especially, are easily influenced, even by repeated small doses of this remedy. He therefore employs opium without any symptomatic indication, after the first days of the disease, with the best success, repeating small doses only so long that the patient sleeps easily, and rather more than he wakes. When, after some days, the disease shows a decided improvement, he confines the use of opium to the night. He compares this process with that of delirium tremens, which, according to his conception, also constitutes a process of intoxication *sui generis*; only he recommends not to force sleep by so heroic doses, and to pay due consideration to all contra-indications against the remedy which may occur in the course of typhus; as such he points out conditions of sopor and of real debility. He believes, that in most cases of typhus abundant opportunity for the proposed method is presented, as the usual symptomatic use of opium already shows, and urges the practice of this method in the beginning of the disease, in case of moderate intensity. He farther calls attention to the advantage of using repeated small doses as against the larger doses, and expresses a desire that other practitioners might deem his proposals worthy of investigation at the bedside.

[A part of the Review of Medical Literature has been excluded from the present number, for want of space, but will be inserted in the next issue. This Department has been sub-divided in such a manner that a complete review of the entire field of Medical subjects will be presented each half year. The next number will contain a review of Surgical Opthalmic and Dental Literature.

G. B * * *]

Scientific Department.

UNDER THE DIRECTION OF BENJAMIN F. SHUMARD, M. D., ST. LOUIS.

METEOROLOGY OF NOVEMBER AND DECEMBER, 1865.

BY GEORGE ENGELMANN, M. D., ST. LOUIS.

	NOVEMBER.		DECEMBER.	
	1865.	Average.	1865.	Average.
Barometrical Pressure—				
Mean	29″.628	29″.570	29″.604	29″.608
Highest	29″.977	30″.381	30″.268	30″.296
date	5th.	8th, 1856.	14th.	6th, 1856.
Lowest	29″.098	28″.724	28″.906	28″.518
date	30th.	18th, 1857.	11th.	8th, 1855.
Temperrture				
Mean	47°.1	43°.4	30°.4	33°.8
Highest	69°.5	81°.5	62°.0	74°.5
date	8th.	11th, 1837.	2d.	9th, 1861.
Lowest	26°.0	—0°.5	—2°.0	—7°.0
date	5th.	28th, 1845.	21st.	31st, 1863.
Evaporation	5°.6	4°.1	2°.0	2°.7
Relative Humidity	59.7	67.5	79.6	74.3
Rain and melted snow—				
Quantity	0″.00	3″.22	3″.63	3″.21
Days on which it fell	0	8	6	7
Thunderstorms	0	1	0	1
Principal Winds	NW. & SE.	W. next SE.	SE.	SE. next W.
Cloudiness	2.9	5.5	5.3	5.2
Fair days	22	10	10	11
Variable days	8	13	13	13
Days without sunshine	0	7	8	7
Stage of River	8¼-18½ feet.		5-10¼ feet.	

Both of these months were of a very unusual character. November, especially, was warmer, dryer and clearer, less changeable and less stormy than this month almost ever is; no fall of rain was recorded, only some mist several times moistened the dusty surface. The barometer, also, showed an unusually high pressure; the mean temperature was four degress higher than usual, and the fair days reached the uncommon number of twenty-two, while ten fair days is the average for the month.

December was just as changeable and as cold as November was steady and warm; rainy, though not cold weather, set in Dec. 3d. A week afterwards, we had delightful spring weather, when, on the 12th, the wind and weather changed; and, on the 16th, the river was crossed on the ice, and remained firmly closed for almost four weeks afterwards. The coldest day was the 21st, when the temperature fell below zero.

Favorable as the mild and dry weather of November was for the public health, December brought the usual allowance of diseases of the respiratory organs, not rarely of a typhoid character, and other typhoid forms, together with numerous cases of catarrhal affections of the mucous membranes of the intestinal tract; the mortality, however, was below the average.

COMPARATIVE METEOROLOGY OF THE AUTUMNS AT ST. LOUIS.

	Autumn, 1865.	Autumn, 1864.	Autumn, Average.
Barometrical Pressure			
Mean	29″.574	29″.498	29″.568
Highest	29″.977	29″.873	30″.381
Date	Nov. 5th.	Nov. 18th.	Nov. 8, 1856.
Lowest	29″.098	28″.856	28″.724
Date	Nov. 30th.	Nov. 9th.	Nov. 18, 1857.
Temperature			
Mean	60°.0	55°.6	56°.2
Highest	98°.5	102°.5	102°.5
Date	Sept. 10th.	Sept. 2d.	Sept 2d, 1864.
Lowest	25°.0	11°.5	—0°.5
Date	Nov 5th.	Nov. 22d.	Nov. 28, 1865.
Evaporation	4°.9	5.°8	5°.3
Relative humidity	71.3	66.8	68.5
Rain—			
Quantity	5″.93	11″.22	9″.65
Days on which it fell	17	19	21
Thunderstorms	5	10	7
Principal Winds	SE.	SE., then SW. & NW.	SE. next W.
Cloudiness	8.9	4.3	4.4
Fair days	47	44	41
Variable days	38	38	37
Days without sunshine	6	9	13
Stage of the River—			
Mean, feet	11¾	3½	7¾
Extremes, feet	8¼–17	2½–6¾	2½–17

This table is the fourth of the comparative meteorology of the seasons; those of the three other seasons are given on pages 154, 368 and 472 of last volume. The table shows that the past autumn (September, October and November) was warmer, dryer and fairer even than this pleasant season usually is with us. Following on a very wet summer it brought an unusual amount of intermittent fevers and allied forms of disease, in regard to which I refer to previous articles.

COMPARATIVE METEOROLOGY OF THE LAST YEARS AT ST. LOUIS.

	1865.	1864.	1863.	Average of 30 years.
Barometrical Pressure—				
Mean	29″.553	29″.504	29″.552	29″.542
Highest	30″.268	30″.224	30″.082	30.″.479
Date	Dec. 14th.	Feb. 18th.	Nov. 10th.	Feb. 10, 1857.
Lowest	28″.769	28″.856	28″.899	28″.516
Date	Feb. 25th.	Nov. 9th.	Feb. 19th.	Mch. 28, 1859.
Temperature				
Mean	55°.9	54°.7	54°.6	55°.6
Highest	97°.0	102°.5	95°.0	104°.0
Date	July 5th.	Sept. 2d.	Aug. 28d.	July 21, 1860.
Lowest	—2°.0	—22°.5	—7°.0	—22°.5
Date	Dec. 21st.	Jan. 1st.	Dec. 31st.	Jan. 1, 1864.
Evaporation	5°.0	6°.0	5°.5	5°.8
Relative Humidity	70.3	65.1	66.8	66.5
Rain and melted snow—				
Quantity	46″.87	37″.61	40″.45	44″.52
Days on which it fell	110	86	97	93
Thunderstorms	40	29	37	44
Principal Winds	SE. next NW.	SE. next NW.	SE.	SE., next W.
Cloudiness	4.5	4.5	4.8	4.5
Fair days	150	130	118	142
Variable days	169	193	195	174
Days without sunshine	46	43	52	49
Stage of River—				
Mean, feet	18⅛	7¾	9	11 1-5*
Extremes, feet	1¼-26¾	1¼-20⅓	0-18	0-41⅓**

* Average of five years. ** Within thirty years.

The above table exhibits the result of the meteorological observations made in the last three years, compared with the average of the last thirty years, Both atmospherical pressure and temperature are shown to have been in the past year higher than

usual, and higher than in the two years before, though the thermometer did not reach such a high point, nor fall as low, as the year before. The evaporation was less, the relative humidity and the quantity of rain, and the number of rainy days, greater, but the fair days also greater than usual. The meteorological character of the year 1865 may, therefore, be summed up as having been pre-eminently steady, warm and humid, with a good deal of fair weather; all of them peculiarities which would be favorable to the respiratory organs, but produce all kinds of abdominal affections and miasmatic diseases; and such in fact we find the prevailing nosological character of the year to have been.

ATMOSPHERIC ELECTRICITY, TEMPERATURE, AND HUMIDITY.

Observations at St. Louis, Missouri, based on daily observations at 6, 9, 12, 3, 6 *and* 9 *o'clock, from morning till night.* By A. Wislizenus, St. Louis, Mo.

I.—*Monthly Mean of Positive Atmospheric Electricity.*

NOVEMBER.

YEAR.	6 A. M.	9 A. M.	12 M.	3 P. M.	6 P. M.	9 P. M.	MEAN OF MONTH	NO. OF THUNDER STORMS.	PREVAILING WINDS.
1861...	11.1	11.2	10.3	9.6	10.7	6.9	10.0	1	SE. & W.
1862...	13.5	16.3	14.5	10.1	11.6	9.7	12.6	0	W. & NW.
1863...	15.0	15.2	14.3	8.4	9.8	10.2	12.1	0	W. & S.
1864...	6.7	7.6	8.9	5.7	7.1	8.7	6.6	2	SE.
1865...	8.6	13.1	12.4	10.6	9.3	6.3	10.1	0	SE. & NW.
Mean.	11.0	12.7	12.1	8.9	9.7	7.4	10.3	.....	.....

DECEMBER.

1861...	11.1	17.8	15.4	13.3	16.1	12.0	14.3	0	SE. & S.
1862...	15.4	16.7	15.2	12.2	13.4	10.7	13.9	4	S., SE. & W.
1863...	13.8	16.0	11.5	9.4	9.1	9.4	11.5	0	SW. & SE.
1864...	11.3	11.5	9.8	7.6	7.4	5.8	9.0	1	W. & SE.
1865...	7.4	8.5	6.9	6.2	4.9	4.4	6.4	0	SE.
Mean.	11.9	14.1	11.8	9.7	10.2	8.5	11.0	.....	.....

2.—*Monthly Mean of Temperature, Fahrenheit.*

NOVEMBER.

YEAR.	6 A. M.	9 A. M.	12 M.	3 P. M.	6 P. M.	9 P. M.	MEAN OF MONTH.
1861	36.1	42.3	51.1	52.1	49.2	45.0	46.0
1862	35.9	40.6	46.6	48.4	43.7	40.2	42.6
1863	37.3	41.4	48.3	49.3	44.8	41.3	43.7
1864	39.3	42.0	49.3	50.0	46.2	42.9	44.9
1865	39.9	44.1	53.5	55.8	49.5	45.1	48.0
Mean	37.7	42.1	49.8	51.1	46.7	42.9	45.0

DECEMBER.

1861	33.6	36.1	43.8	45.6	41.5	37.4	39.7
1862	35.7	38.6	45.5	46.8	42.8	39.0	41.8
1863	31.8	33.7	38.8	40.2	36 7	34.0	35.9
1864	26.0	27.8	33.5	34.0	31.6	29.8	30.4
1865	26.3	28.0	33.2	34.9	32.0	30.3	30.8
Mean	30.7	32.8	38.9	40.3	36.8	34.1	35.6

3.—*Monthly Mean of Relative Humidity.*

NOVEMBER.

YEAR.	6 A. M.	9 A. M.	12 M.	3 P. M.	6 P. M.	9 P. M.	MEAN OF MONTH.
1861	84.4	74.3	60.0	56.1	66.8	72.6	69.0
1862	83.6	71.0	61.8	57.2	67.9	75.3	69.5
1863	82.4	69.8	57.9	55.9	66.3	72.4	67.4
1864	87.4	79.3	64.4	62.2	72.6	79.4	74.2
1865	79.9	69.7	51.9	45.5	59.7	67.4	62.3
Mean	83.5	72.8	59.2	55.4	66.7	73.4	68.5

DECEMBER.

1861	87.7	81.7	64.8	58.5	71.2	81.7	74.3
1862	88.0	75.7	66.0	62.5	77.0	78.4	74.6
1863	89.8	80.2	70.9	70.8	79.6	85.7	79.5
1864	87.8	77.4	67.5	67.8	74.8	78.0	75.5
1865	84.7	71.7	61.8	59.0	68.3	78.9	70.9
Mean	87.6	77.3	66.1	63.7	74.2	80.5	75.0

Yearly Report of Atmospheric Electricity, Temperature and Humidity of 1861, 1862, 1863, 1864 *and* 1865. By A. WISLIZENUS, M. D., St. Louis, Mo.

1.—*Monthly Mean of Positive Atmospheric Electricity in* 1861, 1862, 1863, 1864 *and* 1865, *at St. Louis, Mo., based upon daily observation, at* 6, 9, 12, 3, 6 *and* 9 *o'clock, from morning till night.*

YEAR.	Jan.	Feb.	Mch.	Apl.	May.	June	July.	Aug.	Sept	Oct.	Nov.	Dec.	Mean of Year.
1861	16.5	12.1	9.8	8.8	7.8	4.0	8.7	8.4	8.0	7.1	10.0	14.3	8.4
1862	12.1	16.0	9.4	10.6	7.5	8.0	2.2	2.8	3.0	7.7	12.6	13.9	8.4
1863	16.9	15.9	13.6	8.8	4.7	2.0	2.8	4.4	4.8	12.5	12.1	11.5	9.2
1864	15.8	11.3	11.0	8.5	5.1	4.0	2.3	0.9	1.8	5.4	6.6	9.0	6.8
1865	12.2	9.5	5.9	3.3	2.4	3.4	2 6	5.9	1.2	5.3	10.1	6.4	5.7
Mean	14.7	13.0	9.9	8.0	5.5	3.3	2.7	3.4	2.8	7.6	10.8	11.0	7.7

2—*Monthly Mean of Temperature and Relative Humidity in* 1861, 1862, 1863, 1864 *and* 1865, *at St. Louis, Mo., based upon daily observations, contemporaneous with those of Atmospheric Electricity.*

TEMPERATURE.

YEAR.	Jan.	Fe b.	Mch.	Apl.	May.	June	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean of Year.
1861	32.2	40.4	44.8	58.1	64.1	76.9	77.5	78.6	69.1	57.9	46.0	39.7	57.1
1862	28.9	30.2	43.2	55.0	69.7	75.1	81.2	80.7	72.1	57.3	42.6	41.3	56.4
1863	36.8	35.7	43.6	57.4	65.5	71.9	77.2	77.5	69.2	48.0	43.7	35.9	55.2
1864	29.2	38.3	40.7	59.4	69.4	78.9	83.5	78.8	72.9	53.1	44.9	30.4	56.0
1865	28.1	38.4	46.7	56.8	68.8	80.7	77.7	78.1	77.8	58.8	48.0	30.8	57.5
Mean	31.0	36.6	43.8	55.7	67.5	76.7	79.4	78.7	72.2	57.0	45.0	35.6	56.4

RELATIVE HUMIDITY.

1861	72.2	63.3	64.5	61.5	66.3	70.8	66.3	69.6	77.3	76.6	69.0	74.3	69.5
1862	85.3	78.9	70.8	67.0	57.3	67.0	66.8	64.3	74.2	67.2	69.5	74.6	69.8
1863	79.2	81.7	68.1	57.2	59.4	67.7	68.6	70.7	68.2	74.4	67.4	79.5	70.2
1864	75.6	62.7	70.0	69.8	56.4	61.5	62.8	69.0	64.1	67.9	74.2	75.5	67.4
1865	74.6	72.0	66.1	66.8	62.1	67.9	77.4	71.7	76.8	74·1	62.3	78.8	70.9
Mean	77.4	70.7	67.9	64.5	60.3	67.0	68.4	69.1	72.1	72.0	68.5	76.5	69.6

3—*Yearly Mean of Positive Electricity, of Temperature and of Relative Humidity of the Atmosphere at the hours of* 6, 9, 12, 3, 6 *and* 9, *from morning till night, based upon daily observation at those hours in* 1861, 1862, 1863, 1864 *and* 1865, *at St. Louis, Mo.*

ELECTRICITY.

YEAR.	6 A. M.	9 A. M.	12 M.	3 P. M.	6 P. M.	9 P. M.
1861	8.5	9.9	9.0	7.7	8.5	6.8
1862	8.9	10.0	9.1	7.3	8.1	6.8
1863	10.5	10.6	10.0	7.5	9.1	7.4
1864	7.9	8.8	7.4	5.4	5.9	5.5
1865	6.4	7.1	6.0	5.3	5.4	3.8
Mean	8.4	9.3	8.3	6.6	7.4	6.1

TEMPERATURE.

1861	48.9°F.	54.9	61.6	63.6	59.3	54.3
1852	48.9	55.0	60.9	62.3	58.0	53.6
1863	47.5	53.6	59.7	61.0	57.2	52.2
1864	48.0	54.1	60.3	62.2	58.1	53.0
1865	50.4	55.8	61.8	63.3	59.3	54.7
Mean	48.7	54.7	60.9	62.5	58.4	53 6

RELATIVE HUMIDITY.

1861	86.4	71.3	69.3	57.2	65.1	77.3
1862	85.3	70.6	60.0	57.5	67.6	78.0
1863	86.8	71.4	60.2	58.0	66.7	77.9
1864	83.9	69.3	57.7	55.0	64.0	74.8
1865	84.7	71.7	61.3	59.0	68.3	78.9
Mean	85.4	70.9	59.9	57.3	66.3	77.4

REMARKS.

The above tables contain a summary of my observations of atmospheric electricity, for five years. They are based upon five daily observations, made with *Dellman's* instruments. As a result of these researches, I have found the following laws:

1. The positive electricity, floating generally in our atmosphere, exhibits a daily periodicity, by two maxima and two minima in twenty-four hours, a first maximum appearing about 9 A. M., a second about 6 P. M.; and a first minimum about 3 P. M., and a second about 9 P. M., which is continued till after midnight.

2. Besides this daily periodicity, there exists a monthly one. The far greater quantity of positive electricity appears in the colder half of the year, and the lesser in the warmer half.

3. There is possibly also a yearly periodicity of eleven years, following the same periodicity that exists between terrestrial and solar magnetism, — between the destination of the magnetic needle and the solar spots. The present period extends from 1859 to 1870, with maxima at these two end points, and with a minimum in the middle, in 1865. The uncommonly low mean of electricity in 1865, which cannot be accounted for by other meteorological phenomena, seems to favor this supposition; but bservations must be continued for many more years to decide hat point.

Atmospheric electricity is, in medical works, so often mentioned as a cause of health and disease, that one might suppose that it was an agent thoroughly known. But of all the meteorological phenomena, there is, on the contrary, none so little understood and studied as atmospheric electricity. The reason of it is, that while many have written and re-written about the influences of atmospheric electricity, and ascribed almost every epidemic or unknown disease to that agency, very few have made regular and reliable observations long enough to warrant safe conclusions in regard to the regular or irregular action of electricity at certain periods. By publishing my observations of atmospheric electricity in a medical journal I have tried to supply this want, and to afford to my colleagues of the medical profession an opportunity not only of becoming better acquainted with the regular actions of atmospheric electricity, but forming comparisons also, and drawing conclusions from changes in atmospheric electricity, upon the character of prevailing diseases.

Editorial Department.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

DIE EPIDEMISCHE CEREBRO-SPINAL MENINGITIS NACH BEOBACHTUNGEN IM GROSSHERZOGTHUM BADEN. VON DR. F. NIEMEYER, *Prof. u. Director der Med. Klinik in Tuebingen, etc. Berlin:* 1865. 8*vo*, *pp.* 72.

(EPIDEMIC CEREBRO-SPINAL MENINGITIS, ACCORDING TO OBSERVATIONS IN THE GRAND DUCHY OF BADEN. BY DR. F. NIEMEYER, *Professor and Director of the Medical Clinic in Tuebingen, etc.*)

Many cases of the grave disease, the subject matter of the above treatise, continue to occur in our own country, and the medical journals of the day are filled with reports of them, and discussions concerning their pathology. The interest in this subject seems not to have abated, and this is the chief apology offered for bringing forward a review of the very excellent little work published under the above title, by one of the most eminent clinical teachers of Germany,—one who enjoys the well-earned reputation of having written the best text-book on special pathology in the German language.

The author has witnessed an epidemic of cerebro-spinal meningitis in the towns of Freiburg, Carlsruhe and Rastatt, and from the experience thus gathered, as well as from conversations with other good observers on their own cases, and the study of numerous histories of cases and autopsies, he arrives at views differing, in some essential points, from those of many of his compeers, which shall be noticed towards the end of this review. These views are set forth in the first section (on "*pathogenesis and ætiology*") with striking clearness and conciseness, and further supported in the subsequent pages, more especially in the masterly

analysis of symptoms, in the section on "*symptoms and progress.*"

The very first case he witnessed coninced the autohr "that epidemic cerebro-spinal meningitis, if an infectious disease at all, belongs to that group of infectious diseases in which all pathological symptoms depend solely upon the anatomic lesions of a single or some few organs, caused by the infection." The author favors a classification of infectious diseases, into: those in which the general affection, and especially the fever, depend, in part, directly upon the absorption of the infecting material into the blood; and those in which the only effect of the infection consists in the pathological alterations of a single or a few organs, and their symptoms. The former group embraces, among others, the exanthematous fevers, typhus, plague, and above all, the malarial affections—"zymotic diseases." The prototypes of the second group are epidemic dysentery and cholera. These groups also differ, in that the former presents peculiar anatomical changes produced only by the specific infecting virus, and by no other agency, *e. g.*, the lesions of the skin and mucous membranes in smallpox, scarlatina, etc.; whereas the pathological products of the other group are by no means pathognomonic: a non-infecting diphtheritic inflammation of the rectum does not differ from dysentery caused by contagion; and after cholera nostras the body presents the same intestinal alterations, the same thickening of the blood as in consequence of Asiatic cholera.

The author's conception of epidemic cerebro-spinal meningitis as a disease of the second group, in which he was confirmed by all cases observed by him, is based chiefly on the fact that all its symptoms are referable, without difficulty, to the lesions of the cerebral and spinal meninges as observed after death. He calls attention to the following points:

"A premonitory stage usually is wanting, and where it is observed, the symptoms are confined to complaint of pain in the head and back,—accidents which already point to changes in those parts in which severe lesions are found after death.—Almost without exception, a violent chill opens the scene.—Immediately upon the chill follow symptoms which prove a serious affection of the membranes of the brain and spinal marrow, beyond doubt.—The fever is by no means more intense than the great extent of the intense inflammation would lead

us to expect; it is, on the contrary, rather less than is usually the case in inflammations of equal intensity and equal extent.—The course of the disease is, as a rule, very acute. If the patients do not immediately recover upon its conclusion, but fall into a prolonged ailment, this again depends, most unmistakably, solely and exclusively upon the *residua* of the meningitis.—Finally, at the time of an epidemic, besides well developed cases of the disease, a greater number also occur, in which a pain in the head and back, lasting for a few days, is the only symptom.—The contrast between the phenomena of a cerebro-spinal meningitis and those of a typhous affection, is so striking that I deem it superfluous to digress in drawing a parallel between the symptoms and course of the two diseases." (pp. 9, 10.)

The results of post mortem examinations compel the adoption of the same view. Of constant occurrence is, according to Niemeyer, an extensive inflammation of the cerebral and spinal meninges, which has *no specific* character: sporadic meningitis presents the same lesions, though only exceptionally equal intensity and extent. Moreover, this is the *only constant* anatomical lesion; aside from accidental complications, all other organs are apparently normal. In all this, cerebro-spinal meningitis differs widely from typhoid forms of disease, where the chief lesion *is* specific, and alterations of texture are found in most other organs. The objection, that we often find the blood in a fluid, uncoagulated state, similar to its condition in typhoid subjects, is met by the experience that a like condition of the blood results from other acute diseases (especially those of the brain), in which not the slightest suspicion of an infection exists Cutaneous eruptions are not constant, and, moreover, the diagnostic value of such signs is greatly over-estimated. The occurrence of herpes is as frequent in pneumonia and some other inflammatory diseases, as in the epidemic meningitis.

The question, whether or not this essentially *local* affection depends on an infection, can be decided only by a careful analysis of the ætiological data and the mode of spreading. In Asiatic cholera, and dysentery, likewise, the infectious character of the disease is not apparent (*as it is in typhus*) from the observation of the symptoms and post mortem appearances of a single case, but only from its peculiar mode of spreading, and the proof, furnished in these very diseases, of extension by the virus contained or developed in the dejections.

Although a few circumstances,—such as its simultaneous appearance in widely separated tracts of country (which would seem to indicate a dependence of the disease on telluric or atmospheric influences), and its almost exclusive occurrence in young persons,—seem to speak against infection, the author, nevertheless, is "far from denying the possibility, or even probability, that epidemic cerebro-spinal meningitis is based upon an infection of the organism." His personal experience, however, favors neither side of the question.

Of 126 patients in this epidemic, 66 were males, and 60 females; 54 were under five years of age, and but 5 were over twenty-five.

The pathological anatomy of acute cases is condensed, on page 25 *et seq.*, in the following resumé:

"1. We constantly find a *cerebral meningitis with purulent exudation into the subarachnoid spaces.* While sporadic menigitis with purulent exudation, is usually confined more or less to the convexity, tubercular meningitis to the base, we observe in epidemic cerebro-spinal meningitis, *a greater extension of the inflammatory process, a simultaneous* affection of the convexity and of the parts at the base.*

"2. While,—in contrast with tubercular basilar meningitis,—a copious serous transudation into the ventricles is almost never found in epidemic cerebro-spinal meningitis, we sometimes, but not often, find *the inflammatory process and the purulent exudation extended upon the ependyma of the ventricles and upon the choroid plexus.*

"3. In the great majority of cases, *a spinal meningitis* accompanies the cerebral meningitis. Its product is partly *contained in the subarachnoid space as a purulent fluid,* partly *infiltrated into the tissue of the pia mater as a gelatinous purulent exudation.*

"4. Usually we find, *in the lower part of the ileum, hyperæmia and occasionally ecchymoses of the mucous membrane, as well as a moderate swelling of the follicles,* the signs of a pretty severe catarrh. This appearance may represent a complication, but perhaps it depends on the treatment during life, on the exhibition of strong laxatives, especially of large doses of calomel.

"5. On the integument we find, very frequently, dried-up *herpetic vesicles,* and sometimes scattered *petechiæ.*

"6. The *blood contains only loose, dark-colored clots,*—no colorless coagula of fibrine."

The third section consists of an elaborate analysis of the different symptoms, with histories of cases interspersed, and con-

* On page 29, Niemeyer reports a case lasting only about three hours, in which a copious purulent exudation was thrown out on the convexity of the brain. This illustrates the rapidity with which such an exudation can be produced.

cludes with a graphic sketch of the course and progress of the disease. We venture to give an abstract of this important section:

"Violent *pain in the head* is among the most constant symptoms of cerebro-spinal meningitis." Even in cases of very rapid course, it was not wanting. (Cases in which the patients, without previous symptom or complaint, were suddenly struck down, and expired, had not been observed in the epidemic of Baden.) In complicated and prolonged cases, also, the patients complain of pain in the head, as long as the sensorium is free; and the great restlessness and jactitation and moaning of patients, when consciousness is already lost, are probably due in great part to the tormenting pain in the head. Convalescents are entirely free from headache. During the epidemic a large number of cases were observed in which persons complained of severe headache, without demonstrable cause or other symptoms, reminding one of the simple diarrhœas so frequent during epidemics of cholera. Although we are still in the dark as regards the origin of pain in the head in cerebral diseases, yet experience teaches that inflammation of the meninges are the diseases in which this symptom attains its greatest violence; and the pain in the head in cerebro-spinal meningitis is of the same character as in the sporadic form.

Pains in the back and neck appear simultaneously with the pain in the head, but they are less severe. Pressure upon the spinous processes usually increases the pain, but not invariably. This symptom is not so constant as the former; and in one interesting case which Niemeyer details, its absence coincided with absence of the usual tetanic contraction of the corresponding muscles. Movements of the spinal column, active or passive, increase this pain considerably. When the course of the disease is very protracted—because the exudation is resorbed slowly—this symptom may continue for weeks. The explanation of pain in the back is not difficult, for evidently the posterior columns or roots are morbidly excited by the inflammation of the meninges.

Painful sensations in the extremities are not constant, nor even frequent; but sometimes a radiation of the pain from the back into the arms or legs is distinctly experienced. These radi-

ating pains are evidently neuralgic, occasioned by irritation of the posterior roots, as proved in a case where they were invariably occasioned by every movement of the cervical portion of the spinal column.

Hyperæsthesia of the skin occurs during the first few days, sometimes lasting throughout. It, of course, gives way to a complete (central) anæsthesia, when stupor supervenes. But a peripheric anæsthesia, also, is observed in some cases, to be referred to loss of excitability of the posterior roots.

Tetanic convulsions of the muscles of the neck and back (often confined to the nape alone) are absent in extremely few cases. This "highly characteristic symptom" appears at the very inception of the disease, and may increase to a high degree of opisthotonus, so as to prevent the patient from lying on his back. Trismus often accompanies the tetanus. There can be no doubt that these symptoms depend on an irritation of the medulla oblongata, and it is probable that in those cases only where the tetanus gives way to relaxation (paralysis?), an œdema or a deeper lesion of texture takes place in the spinal marrow itself.

Epileptiform convulsions are rarer than might be expected from the great extension of the exudation upon the convexity of the brain.*

Numerous cases present no *paralytic phenomena* whatever. In others, however, paralysis of the upper or lower extremities occurs, and not unfrequently more or less pronounced hemiplegia. In two cases Niemeyer observed paralysis of the face, but never in the range of the *pars minor trigemini*, the hypoglossus or

* The author thinks that we are not yet in possession of a sufficient explanation of the fact that these general convulsions, combined with loss of consciousness, are sometimes present, sometimes absent in meningitis. The experiments of Kussmaul and Tenner demonstrate that they may be artificially produced by cutting off the supply of arterial blood to the brain On the other hand, experience teaches that among cerebral diseases, those chiefly produce epileptiform convulsions which diminish the capacity of the cranium,—such as large tumors, hydro cephalus. Niemeyer does not wish to assert positively that these causes operate by compressing the capillaries, but calls attention to the absence of epileptiform convulsions in cases of meningitis not complicated with hydro cephalus. The exudation in cerebro-spinal meningitis, though copious, does not cause anæmia by compression, for the smallest vessels of the pia mater are usually found much injected.

the glossopharyngeus. The paraplegia is probably due to pressure upon the motor roots, or to a collateral œdema in consequence of inflammation of the neurilemma.

Hemiplegia may be owing to partial affection, small hæmorrhages or inflammations of the brain itself, or to preponderance of pressure by the exudation upon one side. The paralysis of special nerves is easily explained, for we often find the exudation at the base to surround these nerves, or even to accompany the nerve-sheaths to their points of exit from the cranium.

Psychical disturbances are wanting in the incipient stage. Soon, however, the patients become immoderately restless; then follows the characteristic, uninterrupted jactitation, and delirium. In the greatest intensity of the disease, the loud delirium ceases, while the restlessness continues, until finally it also gives way to stupor and deep coma. This sequence of psychical symptoms seems to be very constant, although the duration of the several phases varies considerably.

Difficulty of hearing, up to complete *deafness*, on one or both ears, is frequent, and may outlast the recovery of the patient. The auditory nerve being often found imbedded in the exudative masses, the frequency of this symptom is not surprising.

Symptoms on the part of the eye, as ptosis, diplopia, keratitis, are often met with, and easily referred to continuation of the meningitis upon the neurilemma of the nerves involved. Total loss of vision was not observed. The *pupil*, in the majority of cases, was contracted, seldom normal; sometimes, especially shortly before death, one or the other was dilated. The contraction is probably due to irritation of the convexity of the hemispheres (Griesinger),—the dilatation of the pupil with ptosis to paralysis of the oculomotor nerves.

The *respiration* is usually moderate; in the acme of the disease, very frequent. The unequal respiration of tubercular meningitis and acute hydro cephalus is not observed. Death generally takes place with symptoms of an acute œdema of the lungs.

The *pulse* shows an increase of frequency up to 90, 100, 120, and in severe cases to 130–140 beats. Diminution of frequency, down to 60 or 50, is a rare exception.

Nausea and vomiting, evidently sympathetic, in many cases set in during the first day; but prolonged vomiting, as it sometimes occurs in other cerebral diseases, is rarely or never seen in cerebro-spinal meningitis. More or less obstinate constipation generally takes place in the latter stages. The abdomen is usually contracted (as it is almost constantly in tubercular meningitis and acute hydro cephalus), which evidently depends on a tonic contraction of the intestinal muscles. *A swelling of the liver or of the spleen cannot be demonstrated by percussion or palpation.*

The *urine* seldom shows an abnormal composition.

The *integument* presents no constant exanthema; the most frequent is herpes, usually around the mouth, but also on the ears, neck, back and extremities. Some cases of dark roseola spots were observed, transformed, on receding, into petechiæ.

Inflammatory *exudations into joints* occurred in only a few cases.

Febrile symptoms. A severe chill in many cases opens the train of symptoms, but other cases are initiated by light rigors only, or no sensation of cold at all. During the first two days the temperature of the body rises but very little. But shortly before the fatal end it may ascend to 40° C., and more. (Niemeyer is of opinion that, in cases of decided opisthotonus, this elevation may be owing, in part, to the tetanic tension of the muscles.) The author now proceeds to describe (p. 61 *et seq.*), in classic diction, the usual course of the disease, and its more frequent variations, which to extract from, would mar the picture, to translate *in toto* would make us transgress the limits of this article. We confine ourselves to copying our author's concluding propositions:

"1. All symptoms observed in the course of epidemic cerebro-spinal meningitis are deducible without constraint from the purulent inflammation of the pia mater of the brain and spinal marrow.

"2. The modifications of the course of the disease in special cases depend on the variable intensity, and especially on the variable extent of the meningitis, and perhaps in part on its complication with inflammation of other organs."

These sentences specify the author's opinion on the nature of this disease, in judging of which it behooves us to remember that Niemeyer professes to describe it only as it occurred before his eyes, in a single limited epidemic; and it is apparent that the latter differed in some important particulars from the cases of some other localities. We must not forget that Niemeyer has observed no case in which the autopsy did not confirm the diagnosis of a meningitis, simple and pure, without lesions of any importance or constancy in other viscera.

The opinions of the different writers on cerebro-spinal meningitis at present may be classed under four heads: 1. It is a form of *typhus*, with predominence of cerebral and spinal symptoms. 2. It is an infectious *blood disease*, chiefly localized in the meninges, but sometimes also in the lungs, etc., and (in a few suddenly fatal cases,) without all local lesions. 3. It is an infectious disease *always* localized in the meninges. 4. It is a non-infectious, entirely local disease.

The leading advocate of the first doctrine is Murchison (*London Lancet, July*, 1865, *p.* 320.) He bases his belief in the identity with typhus, principally in the dissolution of the blood, the petechiæ, and the frequent occurrence of cerebro-spinal symptoms in certain epidemics of undoubted typhus, as well as the pretended similarity of causation. But the first named is by no means a constant sign in the post-mortem reports of other observers, and Niemeyer explains that it is not rare in non-infectious, acute diseases, which take a *rapidly* fatal course. And petechiæ have been so rare in many epidemics that Dr. Stillé objects to the name of *spotted fever* "for a disease in which a majority of the patients present no spots at all." In the epidemic of Baden, the petechial eruption was decidedly a subordinate phenomenon. A later American writer, Baltzell, (*Am. Journ., Oct.* 1865, *p.* 368,) expresses similar views. The majority of American observers countenance the second doctrine: that it is a blood

disease, differing from typhus; *cf.* Levick, Gilbert, Stillé, (*Am. Journal, July*, 1864,) Black, (*ibid, April*, 1865.) The two former, among many others, favor the name of spotted fever, and lay stress on the infection of the blood. A number of British physicians agree with them. Dr. Sanderson, who has observed the epidemic in Danzig, said, in the Pathological Society of London, "he did not think the disease had anything in common with typhus fever, except in so far as each disease was dependent on a specific poison, (*London Lancet, July*, 1865, *p.* 334.) The same refutation of the typhus doctrine is found in Prof. Hirsch's report on the epidemic of Danzig. (*Deutsche Klinik*, 1865, No. 26.) Prof. Wunderlich, whose source of information was the epidemic in Leipzig, discovers "decisive evidence of the infectious nature of the disease, and an analysis of its symptoms is equally conclusive against its purely local anatomical origin. Its characteristic phenomena may be arranged in two groups: 1st. Those which depend upon cerebro-spinal meningitis. 2d. Those which denote a general derangement of the system." Among the latter he classes the various febrile symptoms, enlargement of the spleen, cutaneous eruptions, (*most frequently herpes*,) hemorrhages, catarrhs and secondary exudations. (*Am. Journ., Oct.* 1865, *p.* 509, *from Archio des Heilkunde.*) Stillé (*l. c.*) more nearly approaches our author in his estimate of epidemic meningitis; so likewise does Black (*l. c., p.* 346,) when he says: "From this it would appear that the nervous involvement is more pathagnomonic than the blood extravasation (*petechiæ*, Rev.,) and that the latter may be regarded only as ocular evidence of blood degeneration from the presence of a zymotic agent, whose chief, and perhaps primary, force is spent upon the cerebro-spinal axis." But he adds: "That this affection differs from ordinary traumatic and sthenic inflammaton of the nervous centres, I entertain not the slightest doubt."

Niemeyer's own opinion, that all the phenomena of cerebro-spinal meningitis, depend exclusively on the anatomical lesions produced by the infection, takes its shape undoubtedly from the peculiar post mortem appearances in the special epidemic which he

studied, and from the symptomatology, which, in those cases, accurately corresponded with them. In *all* cases he found a purulent meningitis; a swelling of the spleen in *none*. Petechiæ were rare; herpes was more frequent. Nor does he stand alone in these observations. Epidemics seem to differ in this respect, and individual cases of the same epidemic as well. In the case of Dr. Jewell, reported to the College of Physicians of Philadelphia, there was a yellow serous effusion in the subarachnoid space and spinal canal; the spleen healthy; petechiæ. In Levick's case, also, the spleen was normal. In the autopsy by Edes, (*Am. Journal, July*, '64, p. 275,) the purulent exudation was marked, the spleen "rather large," which is not unusual in a malarial district, where the case was observed. Sanderson (*l. c.*) says, enlargement with softening of the spleen is *not* a constant phenomenon. A French Physician, Vallin, who has observed the same epidemic in Baden, especially in Rastatt, presents (*Gaz. Hebdomadaire*, 5 *Mai*, 1865), very nearly the same statements as Niemeyer, concerning the anatomy, pus in the meninges and normal spleen. In a case reported by Dr. B. London, of Vienna, a purulent meningitis and a healthy spleen were found. (*Wiener Med. Presse*, 1865, *No.* 32.) Prof. Lindwurm, in the Med. Society of Munich, states that in his cases the spleen was not enlarged. In a subsequent session, Prof. Buhl reported four antopsies, in all of which the purulent meningitis was present, while the spleen was enlarged in but one. (*Med. Chir. Rundschau*, 1865, I, p. 457, *et seq.*)

During the same discussion, Profs. Lindwurm and Seitz asserted the *entirely local nature* of the disease, basing their opinion upon the series of cases observed by themselves; they oppose the view that it is an infectious disease, and the former explicitly denies even the most distant similarity of it with typhus.

When the views and experiences of our *lumina* diverge to such extremes, the question cannot yet be regarded as decided; but it seems that Murchison is nearly solitary in support of the identity of epidemic cerebro-spinal meningitis with typhus, and we would fain follow the majority in denying it.

The remarks of our author on prognosis and treatment are very brief. The mortality of the epidemic amounted to 30 per cent. Hence the prognosis is not so unfavorable as in sporadic and tubercular meningitis. The greatest danger to life exists during the first four days of the attack. In case of recovery, the chances of complete restoration of health are favorable; deafness and lesions of vision are specified as occasional *residua* of the disease. The treatment adopted consisted in energetic application of cold, leeches behind the ears, and calomel; and the author sees reason to recommend this practice. G. B.

THE RENEWAL OF LIFE; LECTURES, CHIEFLY CLINICAL. By THOMAS KING CHAMBERS, Honorary Physician to His Royal Highness the Prince of Wales, Physician to St. Mary's and the Lock Hospital. From the Third London Edition. Philadelphia: Lindsay & Blackstone. 1865.

This is said by the publishers to be a very popular work; but we must confess that, after reading its six hundred and thirty-eight octavo pages, we do not think that our time has been very well spent. There is a story of Sydenham, that he advised a student, who consulted him as to what author he should read on the practice of medicine, to read Don Quixote! Now we do not say that Dr. Chambers' work would not be better for the student than the Spanish romancer, yet we do say, that some of the notions of the author seem very Quixotic.

The "*Renewal of Life*" is a very sounding and pretentious title to begin with. Of course, the patient and the physician, too, would like this renewal of life—Poets speak of rejuvenescent fountains, and the alchymists spoke of the elixir of life and the philosopher's stone, and all that sort of thing. The idea of Dr. Chambers is, that "life" is a great thing and ought to be conserved. In this we agree with him. His idea is, that there cannot be too much life—in other words, that there cannot be too much health. In a word, his doctrine is, that all disease implies enfeebled life; and, as a consequence, that all remedies should be stimulant or life supporting. This is mostly the theory of quacks and nostrum venders. Their "*vegetable* pills" are always invigorating, their "syrups" are always purifiers of the

blood. They always promote digestion and increase vital activity.

Of course, the object of the physician is to promote the health and prolong the lives of his patients; but the question is, are not the functions of the system—are not the vital actions sometimes in excess, as well as sometimes deficient in disease? Is not a lowering and sedative treatment sometimes necessary? So we have been accustomed to think. Is there diminution of life in the bounding hypertrophic heart—in the excited brain of the impassioned orator—in the rapidly moving muscles of the dancers? There is certainly increased vital action. After all, does Dr. Chambers know what he himself means by the word "life." Would it not have been better for him to have called his work a "renewal of health;" for then we could agree with him, that nothing should be given that does not directly or indirectly contribute to such renewal? Evidently, he has used the word "*life*" as synonymous with "*health*;" whereas there is an evident distinction, they do not mean the same thing. The inflamed cartilage has certainly as much life or vital action as the healthy cartilage; but it is not so healthy. And it may be, and often is, necessary to diminish the life or vital activity of a part to restore its health. This the author is obliged to admit in practice, though he denies it in words; for he admits the vitality of blood-letting—now, evidently, to take away blood is to reduce life or vital action. The author perceived this inconsistency, and he argues that, in the cases in which it is good to take blood, it is because that fluid is itself diseased and is doing little or no good to the system; but "*this will never do*," even if the blood is poisoned or depraved, it is the best the patient has to sustain his *life*, and to take it away is to diminish life and contrary to the theory of Dr. Chambers and the steam doctors in general.

We would suppose that the old doctrine that vital activity, or "life" has to be curbed occasionally will continue to have adherents when the book we are reviewing shall be forgotten; and that physicians will continue to bleed and apply ice to head in

encephalitis, notwithstanding the fact attested by revelation and reason that the blood is the "life" thereof.

We have suggested that the author should have called his work the "*renewal of health.*" We make another suggestion, it is this—he might have termed it the "cure of disease." Ah! but this would have been a trite and common sort of a title. It would not have been so taking! Hence, we suppose, the origin of the title and the explanation of the popularity of the work.

We do not think the author's practice is much better than his theories. In rheumatism, he does pretty well. He keeps the patient warm and gives alkalis. We doubt, however, that a linen "shirt bosom" is so dangerous as he regards it. In the treatment of pneumonia we decidedly oppose him, for he says that mercury is poisonous in this disease. Now, we hold with almost the entire profession, that it is beneficial in the second stage and in all exudative inflammations as in iritis for example. If our observation of more than thirty years is worth anything, this point is established. We shall not follow the author through his chaos of notions and prescriptions. They perplex and vex rather than instruct. Just think of combining lobelia and iron together in a prescription; and eagerly stuffing the patient with food to replace the blood which he has just taken by a cupping! The good nursing and care which Dr. Chambers bestows on his patient are all very well; but as to his theories and his prescriptions (though the latter are sometimes well enough) we must express the opinion that they are not valuable additions to what was generally known before—what is *new* is not true—what is true is not new. L.

STIMULANTS AND NARCOTICS — THEIR MUTUAL RELATIONS, WITH SPECIAL RESEARCHES ON THE ACTION OF ALCOHOL, ETHER AND CHLOROFORM, ON THE VITAL ORGANISM. BY FRANCIS E. AUSTIE, M. D., M. R., C. P., Assistant Physician of the Westminster Hospital, &c., &c. Philadelphia: Lindsay & Blakiston. 1865.

This is an octavo of over four hundred pages. That the volume has no merit we will not assert, for it would be difficult to write four hundred pages of matter, most of which consists of

the narration of experiments, without saying something new, or at least reminding the reader of something known before but partially forgotten. That alcohol is a stimulant is admitted by the world at large, professional and non-professional. That when taken in excessive quantities it induces narcotism, or paralysis, is also well known, and has been well known for centuries. But Dr. Austie has conceived the idea—has taken up the crochet, that all stimulants do good to the system—that they promote life and health, whereas narcotics are bad, and hence he draws, or attempts to draw, a line of distinction between alcohol as a stimulant and alcohol as a narcotic. In this he has failed.

All over stimulation is narcotic or paralyzing in its effects. Strike a ball with a certain degree of force and you cause it to bound forward. This is mere stimulation, according to the author. Strike it yet harder and you fracture or break it—this is narcotic, according to the same author. Evidently they are both the same, they differ only in degree. His idea that when alcohol, ether, chloroform, &c., are taken in what are called stimulant doses, are always beneficial, is false; for they may be taken in such doses where they are not needed at all—and the idea that they are always injurious when they induce even slight narcotism, is equally false; for narcotism is sometimes necessary to the well being of the system. The whole work, so far as its theory is concerned, is a vain and pedantic attempt to limit the significance of a few words—to tell what *stimulation*, and *narcosis* and *health* mean, which he might as well have left to the Dictionary—a book which others understand as well as himself. L.

THE USE OF THE LARYNGOSCOPE IN DISEASES OF THE THROAT; with an Appendix on Rhinoscopy. By MORELL MACKENZIE, M. D., Lond., M. R. C. P., &c. Publishers: Lindsay & Blakeston, Philadelphia. 1865. For sale by Keith and Wood, Fifth and Olive, St. Louis, Mo.

This is a well written monograph, excellently arranged and answering the purpose for which it is intended. The author dwells considerably upon the minutiæ of its history and manipulation; and devotes three chapters to these points, a desideratum for the student of laryngascopy.

Without being carried away by the enthusiasm which attends all new discoveries in physical diagnosis, we feel confident that every medical man will appreciate the benefits to accrue from the universal employment of the laryngoscope in diseases of the throat. Auscultation and percussion, even at this date, are looked upon by some of the older physicians as useless refinements; the use of the speculum decried as engendering prurient and vulgar abuses; the ophthalmoscope hooted at as transcendental humbuggery; and laryngoscopy, which reveals the action of the vocal chords in healthy play or lays bare the cause of the numerous ills to which the larynx falls heir, must of necessity encounter the same opposition which has befallen all other ameliorating discoveries made by progressive science.

The laryngoscope, as described by Dr. MacKenzie, is an instrument for obtaining a view of the larynx during life, and consists of two parts: 1st, a small mirror fixed to a long slender shank, which is introduced to the back of the throat; and, 2d, an apparatus for throwing a strong solar or artificial light on the small mirror. For thus projecting the luminous rays, most laryngoscopists employ a second (larger) mirror, which reflects the light of the lamp or the solar rays. When artificial light is employed, this illuminating mirror is slightly concave; when sunlight is used, its surface is plane.

The principle that the angle of incidence equals that of reflection, when rays of light fall on a plane surface, is the only optical law employed in the art of laryngoscopy, and when the smaller mirror is properly introduced into the fauces, and light thrown on it from the larger one, the illumination will be so great that the impinged light thrown upon the larynx will be reflected back to the small mirror and thence to the observer's eye; and the oblique plane of the laryngeal aperture (bounded by the epiglotes, the ary-epiglottidean folds, and the arytenoid cartilages) is revealed, of course reversed from their anatomical order. The practice of the art is at first difficult and somewhat tedious; but if the student will strictly obey the injunctions laid down by the author, he cannot fail to arrive at a perfect practice.

Our space is too limited to make any extracts, and we must refer the reader to the book which is worthy of perusal. To the physiologist the study of the healthy larynx by means of the instrument will well repay any labor devoted to it; and to the pathologist the revelations made of abnormal growths, adematories, swellings, ulcerations, erosions, atony, relaxations, inflamations, etc., are of the highest interest. The boasted, yet oft denied, operation of introducing the probang into the larynx, becomes perfectly facile, guided by illumination; warty growths and other tumors are removed from the vocal chords, and arteroid cartilages by forceps, the ecrasseur, etc. Galvanism is brought to bear upon the relaxed organism, and the lost voice returns with the tonic condition of the parts.

The author says:

"I feel it a duty to remark upon the possibility that the larynx may get too much of local treatment. The laryngoscope has brought this organ so completely within our reach, that we are all exposed to the temptation of being too meddlesome. If we can avoid the error to which I have here alluded, the introduction of the larynxscope will be an unmixed good, both to ourselves and to our patients, and it will be soon acknowledged to be one of the most valuable additions to have ever been made to our means of diagnosis and treatment."

It will be seen that our author is not so special in his views as to exclude every other branch of the profession, but desires simply to make his art a valuable adjuvant. We advise all of our readers who feel an interest in this subject to peruse the work. M. A. P * * *

DICTIONARY OF CHEMICAL SOLUBILITIES. First Outlines of a Dictionary of the Solubilities of Chemical Substances. By FRANK H. STORER; Cambridge: Silver & Francis. 1864.

This superb and complete work, of 718 pages, is a splendid comment on the vast labor and distinguished name of its author. We will review it more fully in a future number, as we have only space at the present to say that it is very valuable; and no intelligent Pharmaceutist or Chemist should be without it. The author has our thanks for a copy. Only a few are on sale. E. P. Gray's Book Store, Fourth and Locust Sts., St. Louis.

From the respective publishers we have received the following new works, which, at an early date, we will review:

WINSLOW ON THE BRAIN AND MIND—Obscure Diseases of the Brain and Mind. By FORBES WINSLOW, M. D., D. C. L., Oxon., &c., &c., &c. Second American from the Third and Revised English Edition. Philadelphia: Henry C. Lea. 1866.

WEST ON THE DISEASES OF CHILDREN—Lectures of the Diseases of Infancy and Childhood. By CHARLES WEST, M. D., Fellow of the Royal College of Physicians, Physician to the Hospital for Sick Children. Fourth American, from the Fifth Revised and Enlarged English Edition. Philadelphia: Henry C. Lea. 1866.

ASHTON ON DISEASES OF THE RECTUM—Diseases, Injuries, and Chalformations of the Rectum and Anus, with Remarks on Habitual Constipation. By T. J. ASHTON, formerly Surgeon to the Blenheim Dispensary, Fellow of the Royal Medico-Chinorgical Society, Member of the Pathological Society of London, Member of the Harveian Society, &c., &c. Second American, from the Fourth and Revised English Edition. Philadelphia: Henry C. Lea. 1866. For sale by Keith & Wood, St. Louis, Mo.

THE PRINCIPLES OF SURGERY. By JAMES SYME, F. R. S. E. Philadelphia: J. B. Lippincott & Co. 1866.

ON WAKEFULNESS, WITH AN INTRODUCTORY CHAPTER ON THE PHYSIOLOGY OF SLEEP. By WM. A. HAMMOND, M. D. Philadelphia: J. B. Lippincott & Co. 1866.

HAND-BOOK OF SKIN DISEASES, FOR STUDENTS AND PRACTITIONERS. By THOMAS HILLIER, M. D., London. Philadelphia: Blanchard & Lea, through J. M. Crawford.

THE STUDENT'S BOOK OF CUTANEOUS MEDICINE, AND DISEASES OF THE SKIN. By ERASMUS WILSON, F. R. S. New York: Wm. Wood & Co., through Peter Smith.

REPORT OF THE COMMISSIONERS OF AGRICULTURE, FOR THE YEAR 1863. Washington. 1863.

RHINOSCOPY AND LARYNGOSCOPY; THEIR VALUE IN PRACTICAL MEDICINE. By DR. FRIEDERICH SEMELEDER. Translated by EDWARD T. CASWELL, M. D. New York: Wm. Wood & Co. 1866.

We have received the following Pamphlets, Reports, etc.:

REPORTS ON THE EXTENT AND NATURE OF THE MATERIALS AVAILABLE FOR THE PREPARATION OF A MEDICAL AND SURGICAL HISTORY OF THE REBELLION. Washington, D. C.: 1865.

FIFTEENTH ANNIVERSARY MEETING OF THE ILLINOIS STATE MEDICAL SOCIETY.

REPORT ON THE USE OF PRESSURE IN THE TREATMENT OF GONORRHEAL AND PURULENT OPTHAMALIA.

A REPORT UPON THE EPIDEMIC OCCURRING AT MAPLEWOOD YOUNG LADIES' INSTITUTE, PITTSFIELD, MASS., IN JULY AND AUGUST, 1864; INCLUDING A DISCUSSION OF THE CAUSES OF TYPHOID FEVER.

EXPERIMENTAL INVESTIGATIONS INTO THE ACTION AND USES OF THE BROMIDE OF POTASSICM.

REPORTS OF THE TRUSTEES AND SUPERINTENDENT OF THE TENNESSEE HOSPITAL FOR THE INSANE.

TRANSACTIONS OF THE TWENTIETH ANNUAL MEETING OF THE OHIO STATE MEDICAL SOCIETY.

THE DOUGLASS PATENT ARTIFICIAL LIMBS.

CATALOGUE OF BOOKS, OLD AND NEW. By STEVENS BROTHERS, London.

CONTRIBUTIONS TO BONE AND NERVE SURGERY. By J. C. NOTT, M. D.

TABLEAU OF THE YELLOW FEVER OF 1853, &c., ILLUSTRATIVE OF THE QUARANTINE QUESTION. By BENNET DOWLER, M. D.

INOCULATION IN PENNSYLVANIA. By J. M. TONER, M. D.

We have received the following Magazines and Journals:

THE PACIFIC MEDICAL AND SURGICAL JOURNAL AND PRESS, January, 1866.
THE CHICAGO MEDICAL EXAMINER, January, 1866.
THE MEDICAL NEWS AND LIBRARY, January, 1866.
BOSTON MEDICAL AND SURGICAL JOURNAL, January, 1866.
THE CHICAGO MEDICAL JOURNAL, January, 1866.
THE CINCINNATI JOURNAL OF MEDICINE, January, 1866.
CANADA MEDICAL JOURNAL, January, 1866.
THE AMERICAN JOURNAL OF THE MEDICAL SCIENCES, January, 1866.
THE MEDICAL AND SURGICAL REPORTER, January, 1866.
THE HAHNEMANNIAN MONTHLY, January, 1866.
THE SAVANNAH JOURNAL OF MEDICINE, January, 1866.
BUFFALO MEDICAL AND SURGICAL JOURNAL, January, 1866.
THE MEDICAL AND SURGICAL REPORTER, January, 1866.
METHODIST QUARTERLY REVIEW, October, 1865.
THE CINCINNATI LANCET AND OBSERVER, January, 1866.
METHODIST QUARTERLY REVIEW, January, 1866.
THE RICHMOND MEDICAL JOURNAL, January, 1866.
THE DENTAL REGISTER OF THE WEST, December, 1865.
THE CHICAGO MEDICAL JOURNAL, January, 1866.
AMERICAN JOURNAL OF PHARMACY, January, 1866.
THE NEW YORK MEDICAL JOURNAL, January, 1866.
THE JOURNAL OF MATERIA MEDICA, New Lebanon, N. Y., January and February, 1866.

MISCELLANY.

The present issue is the first of the third volume of the NEW SERIES of the St. Louis Medical and Surgical Journal, and is offered to our patrons in a greatly improved style of workmanship and material as regards the printing and paper, in which we have endeavored to advance the interest of our Journal without regard to expense. We hope to equal, if we may not excel, our many praiseworthy contemporaries. We have an assurance in the prompt and cordial support of Western physicians and surgeons, manifest in our subscription list, that they appreciate our efforts. We intend to succeed in the accomplishment of this object, and, if our patrons sustain us by continuing their subscriptions and contributions of able articles, we shall not fail.

The present number has been somewhat delayed in consequence of the improvement of the Journal and the change of publishers. The next issue will be promptly mailed early in March and in each alternate month subsequently.

We have received many letters asking for the back numbers (1 and 2) of 1865, which we have been unable to supply, the demand for those numbers having exhausted the issue. Any gentleman having a number that he does not wish to preserve, will be allowed the subscription price for one number, and our thanks.

The Summer Course of instruction in St. Louis will be unusually inviting during the coming season, and probably as thorough and complete as in any eastern city, as the clinical advantages are as great as elsewhere, and under the control of efficient teachers of medicine and the colateral sciences.

We have been more than gratified in the revival of so many of our old exchanges that were discontinued for a time by the pressure of the late war; as also in the commencement of new medical serials in the various parts of the country, which we have acknowledged on another page, and placed among on our exchange list. We will take pleasure in making them tributary to the interest of our patrons.

OBITUARY.

DEATH OF COL. JOHN O'FALLON.

On the 17th day of December, 1865, the death of Colonel O'FALLON occurred—an event which, though not sudden or unexpected—filled with sorrow and mournfulness the greater portion of the inhabitants of St. Louis. Colonel O'FALLON died full of years and honor, and leaves such a noble record of his life and services, that his descendants may well treasure and preserve it as far above earthly goods—the *name* and memory of a *good* and *honest* man.

Colonel O'FALLON was born on the 23d of November, 1791, near Louisville, Kentucky, and consequently was in the seventy-fifth year of his age at the time of his decease. His father, an Irish physician, emigrated to this country in 1774, and during our Revolutionary War took a noble and prominent part on the side of right and liberty. At the close of the war he married the sister of General GEO. R. CLARK, and from that union sprang JOHN O'FALLON. In his early youth, Colonel O'FALLON was remarkable for his judgment, generosity, and a predisposition for military glory. He served during the campaign of General HARRISON against the Indians in 1811 with distinction, and received a captain's commission in 1812. In 1818 he resigned his commission, after serving seven years with credit to himself and profit to his country, and settled in St. Louis, then a small trading post.

In 1821, he was chosen a member of the State Legislative body, and during four years faithfully represented his fellow-citizens at the State capital. In 1827 he was married to Miss CAROLINE SHEETZ, a native of Maryland, and had by this marriage a large family. Liberality, so rarely found in the possessor of wealth, formed one of the dominant traits of Colonel O'FALLON'S character; his public and private charities were innumerable, and in this connection we must specify at least one—his noble generosity in erecting the "St. Louis Medical College." His philanthropy, combined with his unostentatious manners, rendered him the beloved friend and guide of the many who now mourn his death. In respect to his memory, every civic and religious honor was shown on the day of his funeral. The Faculty, and a greater portion of the students of the St. Louis Medical College, attended the obsequies, which were held at St. George's (Episcopal) Church, and, followed by the sorrow and tears of thousands, the remains of JOHN O'FALLON were borne to their last resting place in Bellefontaine Cemetery.

In concluding this brief and imperfect obituary, we can truthfully write over his grave this epitaph: "Here lies the body of a Christian gentleman."

JAMES M. CRAWFORD.

JAMES M. CRAWFORD, an estimable and public spirited citizen, proprietor of the well known Book Store, in this city, died on Saturday, Feb. 10th, after a very short illness; the ultimate cause of his death being Congestion of the Brain.

Mr. CRAWFORD had been for many years a book seller in St. Louis, and by his promptness and enterprise, together with his accommodating manners to the public, had rendered himself one of the most popular and successful men in that business.

He leaves a devoted wife and one child, together with many friends, to mourn his early demise.

Receipts of Subscriptions for Vol. III, 1866, not previously acknowledged by letter or otherwise:

Dr. Jno. P. Frothinger, Louisville, Mo.
" J. C. Ford, New Florence, Mo.
" G. W. Mothershead, Kiddar, Mo.
" B. Buren, Old Mines, Mo.
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" T. J. Means, Bardolph, Ill.
" J. W. Evans, Pleasant Vale, Ill.
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We cannot refrain giving the following specimen of correspondence from one of the *ignorami*, and to say it is useless to burthen journalists with such requests:

AUGUST 10the 1865

Mr F W White M, D

I have bin Bured, out of all I have in this world and lost my Diploma to I Professor of the Theory and Practice of Medicine graduate of Jefferson Medical college of Philadelphia I Requst you to Right to mee and let mee know if you will give me. a Diploma or blank so I, can have it fild out here if you will send me one after you Right to mee the pise of it I will Send you a Check on eny Bank you wont for the a mount In gold coin

Pleass Right to mee as sune as you get this and Direct your letor

* * * * * *

MORTUARY STATISTICS.

Abstracts of Deaths occuring in the City of St. Louis, for the nine weeks ending December 31st, 1865.

WEEK ENDING	SEX.	AGES. Under 5 Years.	5 to 100.	10 to 15.	15 to 20	20 to 25.	25 to 30.	30 to 35.	35 to 40.	40 to 45.	45 to 50.	50 to 55.	55 to 60.	60 to 65.	65 to 70.	70 to 75.	75 to 80.	80 to 85.	85 to 90.	90 to 95.	95 to 100.	100 to 105	105 to 110	Total
November 4.....	Males....	27	1	..	1	5	6	2	8	4	4	1	..	..	1	1	..	..	1	..	..	..	..	62
	Females .	24	9	..	1	3	6	3	4	1	2	..	2	..	..	3	2	2	..	..	..	..	..	62
" 11.....	Males....	23	3	1	2	7	2	2	5	4	7	2	1	1	1	..	1	..	1	..	..	..	..	63
	Females .	27	4	1	7	..	2	1	2	..	1	3	..	..	1	3	..	..	..	..	..	..	..	50
" 18.....	Males....	16	3	1	2	6	5	5	2	5	3	1	..	3	1	1	..	..	..	..	..	..	..	56
	Females .	29	4	1	..	1	1	1	1	3	3	..	1	..	2	..	..	..	..	..	..	..	..	47
" 26.....	Males....	29	2	1	3	4	3	4	6	3	2	2	2	2	1	1	..	..	1	..	..	..	..	66
	Females .	19	1	2	2	4	4	3	1	1	..	1	..	..	1	2	..	2	1	..	..	..	..	44
December 2.....	Males....	31	2	4	3	7	7	6	3	4	7	1	2	1	1	..	2	..	..	..	..	..	..	81
	Females .	28	3	2	2	1	1	3	..	1	..	2	..	1	3	1	1	2	..	..	..	..	..	51
" 9.....	Males....	25	2	1	1	3	5	5	3	5	2	1	2	1	3	2	..	..	1	..	..	..	..	62
	Females .	27	2	2	2	4	1	4	2	1	3	..	2	2	1	1	..	..	..	..	..	..	..	54
" 16.....	Males....	29	2	2	3	3	5	5	5	4	2	4	2	2	..	1	2	..	..	..	..	..	..	71
	Females .	10	2	..	..	1	2	2	1	1	..	1	2	1	1	1	..	..	..	..	..	..	..	25
" 23.....	Males....	28	2	..	1	2	3	5	4	4	1	2	1	1	4	2	2	..	..	..	..	..	..	62
	Females .	25	1	2	..	..	4	1	4	1	1	1	2	..	2	1	..	1	..	..	..	..	1	45
" 31.....	Males....	26	..	2	1	1	1	2	5	2	2	1	2	1	..	..	1	..	..	..	..	..	..	50
	Females .	19	3	2	3	4	2	4	3	2	1	3	2	1	..	..	..	..	..	..	..	..	..	48
Total.....	.....	412	46	24	34	56	60	58	59	46	41	26	23	17	23	20	11	7	4	1	..	..	1	999

Not included in the above Abstract are 75 Still Born, which, added to the Total, 999, would give 1074 deaths, as seen below in the "Statement of Diseases."

Statement Showing the Diseases resulting in Deaths, for the Nine Weeks ending December 31, 1865.

Abscess, 7; do of Liver, 1; Anæmia, 1; Aneurism, 1; Appoplexia, 10; Asthma, 1; Cancer, 8; do of Uterus, 1; Cancrum Oris, 1; Casualties, 12; Burns and Scalds, 8; Fracture, 4; Suicide, 3; Cholera Infantum, 2; do Morbus, 2; Cerebro Meningitis, 1; Congestion of Brain, 27; do Lungs, 19; Consumption, 97; Convulsions, 51; Croup, 71; Cyanosis, 1; Debility, 85; Diabetes, 1; Diarrhœa, 62; Diptheria, 53; Disease of Heart, 17; Dropsy, 20; Dysmenarrhœa, 2; Dysentery, 35; Dyspepsia, 1; Epilepsy, 3; Erysipelas, 4; Exhaustion, 2; Fever Bilious, 4; do Congestive, 2; do Pueperal, 5; do Remittent, 5; do Scarlet, 12; do Typhoid, 63; do Typhus, 2; do Nervous, 8; Gun-shot Wound, 5; Gangrena, 4; Hemorrhage, 2; do of Bowels, 1; do of Lungs, 8; Hooping Cough, 4; Hydrocephalus, 3; Inanition, 1; Inflammation of Brain, 18; do of Bronchi, 9; do of Kidney, 1; do of Larynx, 13; do of Liver, 11; do of Lungs, 110; do of Pleura, 5; do of Stomach and Bowels, 19; Meningitis, 14; Mania a Potu, 6; Marasmus, 25; Melanoses, 1; Noma, 2; Not stated, 14; Old age, 9; Parturition, 1; Peritonitis, 1; Paralysis, 4; Pyemia, 1; Paralysis of Lungs, 1; Scrofula, 8; Spasms, 21; Still born, 75; Unknown, 4; Varicella, 1; Trismus, 11; Teething, 4; Tetanus, 5; Ulceration of Throat, 4. Total, 1,074.

ALEX. MONTGOMERY,
Clerk of the Board of Health.

THE

St. Louis Medical and Surgical Journal.

FOR 1866.

BI-MONTHLY.

TERMS, INVARIABLY IN ADVANCE.

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☞ Original Communications, to be admitted into this Journal, must be communicated to it alone; for if contributed to several Medical Journals simultaneously, they will be considered in the light of Advertisements. We desire our columns to afford the Profession generally the benefit of such facts as are not published in other Medical Journals, coming under the observation of our Contributors.

All Communications and Correspondence must be written upon one side of each page, in order to insure their insertion.

All Communications, and Books for review, should be sent free of expense; and all requests for information regarding matters of interest to the writer, should be accompanied with the usual postage.

As a channel of communication to Physicians in the West, Northwest and Southwest, this Journal offers peculiar advantages for advertising Medical Books, Instruments, Colleges, Drugs, Medicines, etc., etc.

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FRANK W. WHITE, M. D., Box 3060,
St. Louis, Mo.

THE SAINT LOUIS

Medical and Surgical Journal.

MARCH AND APRIL, 1866.

Original Communications.

OBSERVATIONS AND SUGGESTIONS ON EPIDEMIC CHOLERA.

BY EDWARD MONTGOMERY, M. D.

The word CHOLERA comes home to us at this time with dread significance, as that awful malady has again invaded our continent. Whether the dire epidemic will this time traverse the length and breadth of our land, and spread death and dismay in its course, is only known to Omnipotence: but "to be forearmed is to be forewarned," and it is the solemn duty of those who have passed through two epidemics to cast a retrospective glance at the means and measures adopted to prevent or mitigate the horrors of the pestilence. It is much to be feared that cholera, like scarlet fever, smallpox, etc., will soon become one of our common epidemics and frequent visitors. It was not until A. D. 1817 that

we had any truly scientific account of the disease; and it was fourteen years from the terrible epidemic in 1817, in India, before it reached the British isles. In seven years (from 1817 to 1825) it traveled from the island of Bourbon to the Caspian Sea, and from the Philippine Islands to Asia Minor, but as I have stated, did not reach England until October, 1831, or this country until June, 1832. Its march is fearfully rapid, and it is more than probable it will soon become a common disease throughout the civilized world. Scarlet fever is said to have been unknown to civilization until the seventeenth century; and smallpox was unknown in Europe until the beginning of the tenth century, and was carried to this continent by the Spaniards, twenty-five years after its discovery by Columbus. Judging from analogy, then, we have great reason to fear that epidemic cholera will soon be common amongst us; and in this view it is particularly incumbent upon physicians to devote their best energies and profoundest thoughts to its prevention, mitigation or cure. It is seen that it raged in India under a heat of 100 degrees, and in Russia with the thermometer at zero. Its victims have been gathered from the sea-girt isles of the Indian Ocean, and from those of the North Atlantic; from the low and humid valleys of the Ganges and the Nile, and from the sandy deserts of Arabia; the elevated plateau of Caucasus, and even from the bases of the Hymalaya Mountains, 8,000 feet above the level of the sea. It flourishes alike in the salubrious and temperate isles of Great Britain; in Italy, with sunny skies and balmy airs, and in *la belle* France; in the cold regions of Russia, Prussia and Hungary, and in the torrid climes of India, and even to the 20th degree of south latitude. It flourishes

> "Where the north-east winds blow Sabean odors
> From the spicy shores of Araby the blest,"

And where

> "The lowering elements
> Send o'er the darkened landscape snow or showers."

Still it is an admitted and palpable fact, that in low, humid, warm localities it has always been much worse, *cæteris paribus*,

than in cold dry regions. There have been numerous theories of the aetiology of epidemic cholera; ochlesis, ozone, fungoid and animalcular, electrical, telluric, atmospheric, etc.; and it is probable that a strict and prejudiced adhesion to one of these theories has greatly retarded, or, indeed, completely ignored a rational and judicious course of preventive and remedial measures. There are many strong arguments in favor of ochlesis or overcrowding, as being the chief cause, such as its so frequently occuring in crowded cities, in armies, in the crowds of Mohamedan worshippers, etc. Atmospheric causes have also many believers, and there are some facts which have apparently pointed in that direction. Colonel Hamilton writes, that he was encamped, one evening, on the low flat rice fields of India with a portion of a British regiment of soldiers; that suddenly a black heavy cloud overspread the camp like a funeral pall, there was a very disagreeable and nauseous odor distinctly perceived in the atmosphere, and whilst the men were fearfully conjecturing and discussing the nature of the phenomenon, a cry of anguish arose and twelve of the men were in the agonies of cholera.

In November, 1848, two ships left the port of Havre, which place was said to be entirely free from cholera; the ship Swanton was bound for New Orleans, and the ship New York for our Eastern metropolis of that name; when out in mid ocean, and probably not very far apart, the cholera broke out on board each ship, and some twenty or thirty died in each vessel before reaching the destined harbor. Did these ships pass through a belt of poisoned atmosphere in the middle of the rough Atlantic ocean; were the germs of the disease remaining dormant in the systems of the passengers, to be aroused into active life by the tossing of the waves and the cold moist sea air; or did the pestilence spring forth from some ferment or local infection on board the ships? These questions are difficult to solve, although it is likely a majority would be inclined to believe the first supposition.

It is well known, that, during both our epidemics, the air was very close, heavy, damp, debilitating, whenever the mortality was greatest. I well remember reading in foreign papers, as well as

our own, that wherever the malady was raging, the weather and atmosphere were represented as being of the character indicated.

In the epidemic of 1832, the city of New Orleans suffered terribly for a short time; the disease suddenly broke out there in October, in the midst of a yellow fever epidemic, and there were five hundred deaths per day from cholera for a few days during its height. Six thousand victims fell in about twenty days in a population of about fifty thousand souls, when a friendly visit of a norther suddenly dispelled the disease.

In 1849, the epidemic became more or less severe in New Orleans in strict accordance with the state of the atmosphere and weather, and in St. Louis the same facts were observed. Although there were some sporadic cases here in January, February and March, it was not considered an epidemic until April; in the last week of that month we had forty-three deaths from cholera alone, and the following week eighty-one deaths; the week ending 14th of May, one hundred and eighty-nine deaths. On the 17th of May, the great fire occurred and there was a great diminution of cases and of death, until the second week in June. The period of the greatest mortality in this city was from the 15th of June until the 19th of July. In the seven days from June 23d to 29th inclusive, seven hundred and sixty-six died of cholera alone! It may be here remarked as rather singular, that the mortality from other diseases was not lessened during the cholera epidemic; for, during the above mentioned dismal week, the number of deaths from all diseases was nine hundred and sixty-two.

During all the time of our greatest mortality it was universally remarked that the weather was sultry and the air close, heavy and enervating; and I well remember, about the last days of July, a good fresh breeze enlivened us, and the pestilence suddenly abated.

In 1832, the city of London only lost 5,000 by cholera, whilst, in 1849, there were 19,000 victims; the increase in the latter year was generally attributed to overcrowding and bad sewerage, and great filthiness of the city. In the last week in August, 1849, the deaths from cholera in London were 2,796; this was

their dismal week, and I remember the English newspapers complaining of the oppressive debilitating atmosphere at that time. Whether the atmosphere is rendered poisonous by the bad condition of its ozone, or by containing animalculæ or fungi; or whether we receive the infection into our systems by the food we eat, the air we breathe, or the water we drink; all are agreed that something poisons our blood, depresses or paralyses our nervous systems and suspends or destroys digestion, secretion and absorption. This seems its real nature—its true pathology. The fluid parts of the blood exude through the coats of the alimentary canal, and is generally discharged by vomiting and purging; the thick viscid blood that is left cannot well circulate through the capillaries and pulmonic vessels; the heart's action is impeded, suspended or destroyed; the lungs cannot effectually produce the chemical changes on the blood so essential to its integrity; the liver cannot secrete, digestion is effete, absorption ceases, the kidneys fail to act, and the whole vital powers are suddenly prostrated. These are the links of the morbid chain which constitute cholera. And whether the specific poison first exerts its destructive powers on the nerves, the blood or the alimentary canal, all are ultimately paralised; and it seems rational and philosophical that we should direct our preventive and remedial measures with a view to meet all these indications.

Cleanliness, free ventilation, good pure water, wholesome and suitable food, and temperance, are recognized, by both contagionists and anti-contagionists, as the main preventives; whilst filth, over-indulgence, crowded places, impure water, unwholesome or unsuitable food, low, damp, badly ventilated places, are as generally acknowledged to greatly favor the rise and spread of the disease. These are the nurses to hatch, if not the parents to generate the contagion.

In all crowded and badly ventilated places the occupants should be removed to airy, healthy quarters, for it is attested that in the army in India, and amongst the crowds of pilgrims on the banks of the Ganges, numerous victims of the disease who were considered entirely hopeless and beyond the reach of remedy, on

being carried out to some isolated place to die, recovered. Strict temperance should be enjoined on all; and by temperance here I mean the avoidance of every excess. Let the moderate drinker continue to drink, but to be still more moderate; let the total abstainer remain so, and, in short, let our habits and modes of life, in eating, drinking, exercising both body and mind, be guided by temperance and prudence, cultivating cheerfulness and equanimity as much as possible, avoiding late hours and every excess of mental or corporeal labor. As far as it can be accomplished, plain, wholesome food and pure water should be provided for the poor; and every nuisance should be abated. The free and general use of disinfectants, such as chloride of lime, common salt, sulphate of iron, sulphuric acid, tar, creosote, sulphur, and the deservedly popular disinfectant composed of common salt, binoxide of manganese and vitriol, will be of immense service in preventing the breaking out of the disease, or in mitigating it if already existing.

Numberless proofs could be given to show the great efficacy of sanitary measures in preventing or curtailing the ravages of cholera. The town of Nottingham, England, lost three hundred in the epidemic of 1832, and only five or six in 1849, owing to the judicious preventives employed in the latter year. We are informed that on the banks of the Scinde 5,000 died in five days, in a population of 90,000, but on the people removing to a more salubrious locality, the plague was stayed. In 1793 the assembly of the Hindoo worshippers were seized with the cholera on the banks of the Ganges, and 20,000 victims fell in the course of eight days! Whilst in the town of Jawalpoor, only seven miles distant, not a case occurred.

But the most awful ravages of the disease on record is that of Mecca, in 1831: it is said that three-fourths of the immense crowd of pilgrims assembled there were cut off in a few days! These poor Mahomedans were exposed to the night air, in a low, humid, close atmosphere, enduring the wet chill of night, and the torrid glare of day, terribly filthy and crowded together, with impure and unsuitable food, etc. Forty-thousand persons died in

the town of Benkok, kingdom of Siam, in 1817, whilst Bassora and Bagdad, near the Persian Gulf and river Tigris, suffered frightfully, all these places being notoriously destitute of every good sanitary arrangement. In 1832 Paris lost 18,000, and London only 5,000; and the medical men of both cities attributed the greater mortality in the former, to the filthy condition of the city and the want of good water for the poor. Ireland only lost 22,000 out of her population of eight millions, and Dr. Graves and other high authorities attributed this small mortality to the wise sanitary measures adopted, and the benevolent exertions and judicious attentions of the physicians, clergy, and people generally, to the wants of the sick and suffering.

Hungary suffered more than any country in Europe in 1832, and this was accounted for from her low wet soil, and humid atmosphere, but particularly for the slovenliness and filthiness of her inhabitants. Jassey in Northern European Russia and Halle in Germany both suffered severely, and the reason assigned that both places were greatly overcrowded with a motley mixture of Jews, Gipsies and outcasts from all nations, most of them exceedingly irregular, imprudent and dirty in their habits.

In the prison of Ebrucht, Bavaria, fifteen per cent. of the three hundred and fifty inmates died, whilst in the prison of Kaishan, Bavaria, the epidemic broke out about the same time and only one died of the six hundred inmates, owing to the free use of disinfectants, good ventilation and other sanitary measures. In a village in England, where there was no cholera or even diarrhœa at the time, a house containing 1,400 pauper children lost one hundred and eighty by the disease, owing to the foul sewage, over-crowding and bad ventilation of the poorhouse. The town of Exeter, England, lost four hundred and two cases in 1832, and only thirty in 1849, sanitary measures having been totally neglected in the former, but timely attended to in the latter epidemic.

The medical officers employed by the British and French Governments in 1848-9 to investigate the causes, nature and treatment of the disease, gave it as their opinion that timely

and judicious sanitary measures, properly employed, would divest the malady of a great portion of its terrors and destructiveness. Dr. Sutherland, the chief of the English Bureau of Medical Inspectors, reports that, after inspecting forty or fifty towns, the ravages of the disease were palpably and eggregiously worse where worse sanitary conditions obtained. In our own country we have abundant evidence of the great advantages derived from wise preventive measures; in our own city, in 1849, it is said we lost nearly ten thousand from the epidemic; and we all know that our sanitary condition was then lamentably bad in every particular. The great city of New York only lost 5,091, Boston only 633, Philadelphia only 747, and Baltimore but a few cases, in the epidemic of 1849. The small mortality in these four cities is justly attributed to their favorable hygenic conditions.

In regard to quarantines and cordon sanitaires, I believe a majority of the best physicians in Europe and in this country are sceptical about their efficacy and nsefulness. It is probable that if the labour and means expended on them were applied to ameliorate the moral, social and physical condition of the people; preventing all over-crowding, furnishing plenty of pure water and wholesome and suitable food, removing all filth and nuisances, prohibiting intemperance, rectifing all foul and imperfect sewage, a free use of disinfectants and prompt medical attendance on all cases, much more good would be accomplished. It is well known that the European Governments, as well as the authorities in this country, tried to prevent the advance of the epidemic prior to 1832, and again in 1848; but with what success we are all aware. Cordons sanitaires were established on all thoroughfares, and quarantines organised in all ports to stop the advance of the pestilence; but the Angel of Death crossed over the mountains, descended the valleys, traversed the seas, and undelayed, unobstructed, pursued its course in defiance of quarantines and sanitary cordons.

We also know that many towns and villages escaped, although in constant and uninterrupted communication with places where the disease was raging. Marseilles suffered terribly, whilst Lyons

escaped, although a thousand persons fled from the epidemic in the former to the latter place for safety. Birmingham escaped, whilst Bilston, only eight miles distant, suffered more than any other town in England, according to population, and yet there was hourly communication between Birmingham and Bilston, during the whole time.

On the other hand, it is argued by those who advocate quarantines and cordons sanitaries, that the city of St. Petersburg twice escaped the pestilence by these measures. At one time, cholera was at Tishuria, within one hundred miles of the city of the Czar, but was kept at bay. It is also alleged that these measures prevented the advance of the disease into Europe from Asia Minor, and that the immunity enjoyed by Australia was because of non-intercourse with infected districts, although the disease was in the Philippine Islands and other places in the Indian Ocean.

It is probable that cholera is contagious as typhus fever is contagious; or in other words, that it will spread and become more malignant if the higienic and sanitary condition surrounding it is bad; and we may arrest its course or disarm it of its terrors, by opposing to it, not quarantines and cordons sanitaries, but a temperate, cheerful, cleanly people, free ventilation, plenty of pure water, suitable wholesome food, clean streets, alleys and houses, good sewerage, and active benevolent committees of vigilance to search out and visit the poor, afflicted, and destitute, to furnish them with proper sustenance, disinfectants and timely medical attendance.

The ills which spring from the bosom of Nature can generally be remedied by a wise observance of Nature's laws.

[To be continued.]

ON THE USE OF ATROPINE IN CORNEAL AFFECTIONS.

BY MONTROSE A. PALLEN, M. D.

When in Berlin, in 1857, the great advantages of mydriatics in affections of the iris, were several times pointed out to the writer, by that great ophthalmist, Dr. A. Von Graefe, who was then pursuing his remarkable researches on *Iridectomy, in exclusion of the pupil; irido-choroiditis; glaucoma, and the glaucomatous process.*

In the "archiv für ophthalmologie," Bd ii, Berlin, 1856, we find in extenso the mongraph of Graefe on *iridectomy* as a curative treatment in *chronic iritis and irido-choroiditis*, and he bases such treatment upon purely physiological data, which consists in producing relaxation of the tissue of the iris, the escape of exudation behind it, and the discharge of blood from its vessels and from those of the choroid.

Before detailing some cases of corneal affection treated by mydriatics, it is proposed to examine into such affections, and the object of these remarks is to show that diseases of the cornea are never primary, save when traumatic, and that they are induced by defective nutrition, which is remedied by mydriasis.

Analogy leads us to deduce from certain vascular conditions of the surroundings of the cornea, that mydriasis operates in a mechanico-pathological manner, as does surgical interference, such as is alluded to by Graefe on iridectomy. To conduce to such a conclusion, two propositions are hypothecated.

1. The cornea proper is never primarily idiopathically affected, as disease is caused in it by interference with its nutrition, either by a stasis of blood in the canal of Schlemm (a circular sinus running between it and the sclerotic), or by distension of the vessels of the uvea, embracing the choroid, the tensor muscle, ciliary processes and iris; the ulcerations of the cornea produced

by destruction or disease of the trigemeninal nerve, being secondary.

2. Mydriatics, like excision of a piece of the iris, necessarily influences the secretion of the aqueous humor, the chief nutrition of the cornea, and a compensation is almost probable, by a diminution of the intra-ocular pressure, which produces the stasis in the canal of Schlemm, above noticed, and the distension of the surrounding vessels.

With regard to the first proposition, that the cornea is never primarily affected idiopathically, but that disease in it is a sequitur, it is proposed to attempt such demonstration by an examination into its physiologico-anatomical and pathological conditions.

The cornea is composed of one layer, all others mentioned by anatomists being produced by boiling water and other reagents upon its chondrin. Blood-vessels do not exist in the cornea proper, its nutrition being carried on by endosmosis and exosmosis; its structure is of stratified bundles of fibres analogous to those of the areolar tissue, and between which there exists an amorphous solid and transparent matter. Anterior to the cornea is the conjunctiva, erroneously presumed by some to be the first layer of the cornea; it consists simply of stratified tessellated epithelium and is continuous with the conjunctiva scleroticæ, and wants only its cellulo-vascular basis. Behind the cornea is a very solid amorphous membrane (the membrane of Descemet or Demours) covered by a pavement epithelium, which is carried over or brought from the iris. Schlemm has traced branches from the ciliary nerves into this membrane.

The question now arises, the cornea being a tissue of no very highly organizable qualifications, a secondary substance depending upon its surroundings for its nutrition, whether or not it can specifically of itself take on a primary inflammatory process. Cartilage is the analogue of the cornea; and when its texture is normal it never takes on primary inflammation, but still may seriously suffer in texture from the effect produced on it by inflammation of adjoining tissues, and by the products of that

inflammation, and even from imperceptible anomalies in the composition of the synovial fluid. So says Rokitansky. How can cartilage or corneal tissue take on primary inflammation when it possesses no blood-vessels, and into which we have no evidence of the penetration of lymph, unless we accept the doctrine of the cellulo-pathologists, that inflammation consists in an alteration of nutrition, a change in the act of nutrition, here regarded as embracing the formative and nutritive process; and that, in consequence of some external cause, the part falls into a state of irritation, and its composition or constitution undergoes alterations which changes its relations to the neighboring parts, and enables it to absorb from them a larger quantity of irritaments than usual, and transform them according circumstances; in other words, that inflammation is not to be considered as a real entity? To accept this doctrine we must discard the old theory that inflammation is increased action, increased weight and bulk, hyperæmia, more acute sensibility and greater temperature, followed by exudations of the fibrinous and plastic nature, or aplastic, as the case might be. Such is the character of the lesion denied as primarily existing idiopathically in the cornea; and as such, most, if not all writers up to a very short period have considered the pathological basis of the disease known as corneitis or keratitis; and the therapeutics deduced from such premises have been wofully unsuccessful.

Observations made by Strube in Wirzburg, in 1851, and under the superintendence of Virchow, have developed the fact, that changes in the cornea due to inflammation, are not lymph exudations, but an alteration of its proper constituent texture, consisting principally in a swelling and enlargement of its corpuscles, with minute fatty molecules in them and the increase and enlargement of their nuclei; whilst the intercellular matter becomes opaque, turbid, denser, more fibrinous and granulated, and sometimes with fatty molecules also. Virchow states, that these results are extremely like those in *arcus senilis*. Another fact worthy of notice is, that the cornea never undergoes the exudative process previous to destruction. Having thus premised the

pathological status of corneitis, what is its etiology? A patient being thus attacked, what are the signs revealed upon examination of the eye? There is a certain haziness observed in the centre; then it extends towards the circumference, when it looks like chipped glass; then it assumes a dirty white color very much like a piece of flint; and there is always more or less of redness and injection of the circum-corneal vessels of the sclerotic and conjunctiva, particularly in the former, giving rise to what is known as the *circum-corneal zone*. These symptoms exist in a greater or less intensity. The physiological symptoms are marked; in some patients there is great photophobia, in others not so much, in proportion as the irido-choroid-retinal tissues are affected. Sometimes the digestive apparatus is seriously deranged —with strumous patients the skin is pale, flabby and dry—the sub-maxillary lymphatic glands are engorged; and in females there is sometimes a marked coarseness of voice. The progress of the disease previous to the treatment by the bold use of mydriatics was very slow and its duration very long. Desmarres mentions one case as having been under his treatment for eleven years, and he feared at that time, May, 1855, that it was an incurable one. The prognosis is of course grave, if the old plan of mercury, bloodletting, astringent collyria, opium, etc., be employed, as recommended by the standard authorities on the subject, viz., Desmarres, MacKenzie and Lawrence; but should the treatment be based upon the truly scientific methods, or rather, if it be carried out by a process of analogous reasoning from Graefe's researches in iritis, irido-chroiditis, etc., then the prognosis is more favorable; so much so, that it may not be considered too sweeping to say, that if a case of affection of the cornea be seen early, and the bold use of mydriatics persisted in, the constitutional nutritive plan accompanying it, that the termination will be resolution in fully ninety per cent., and that spots will be rarely seen, staphylomata rarer still, amaurosis almost never, and synechia never. The treatment, as far as local applications are concerned, is mainly and principally mydriatic, consisting in collyria of the neutral sulphate of atropine of the

strength of from two to eight grains to the ounce of water; examples of which will be given further on.

It is now proposed to show how the atropine effects the eye, and why such action influences the cornea towards a resumption of its healthy functions, by promoting absorption of the detritus of disease. The instillation of a solution of atropine into the eye, if such be in a healthy condition, in a very few minutes causes a rapid and complete dilatation of the pupil, and which ordinarily exists for about eight days, sometimes much longer. Why such a condition ensues is inexplicable, but that it does is an accepted fact. If a pupil be thoroughly dilated by atropine, what is the physiologico-anatomical conditions? First, there is immobility of the iris, which is contraction of its dilator muscular fibres, and dilatation of the sphincter iridis or contractor muscular fibres; secondly, vascular condition is decidedly changed, by a diminution of the calibre of the vessels and of the quantity of blood flowing through them. Now, as the aqueous humor is formed principally by exosmosis from the uvea, which is connected with the posterior surface of the iris, the tensor muscle and the ciliary processes, any action which tends to diminish their exosmotic productions also tends to relieve any intra-ocular pressure produced by such, which is done by diminishing four-fifths of the surface, throwing out the aqueous humor. A question naturally presents itself here: what becomes of the aqueous humor, expressed from the surface producing it? A very large portion of it is taken up by the membrane of Descemet, thence for the nutrition of the proper layer of the cornea, and carried off after its nutritive functions are performed, by the canal of Schlemm.

Another point in the anatomy of the circum-cornea region is, that the iris is not inserted just at the junction of the cornea with the sclerotic, but a little behind, at the termination of the internal surface of the ciliary ligament and the ciliary processes; and furthermore, that all of the fibres of the iris here do not commingle with the ciliary ligament and processes, but, according to Graefe and Jaegr, jr., some of them bend forward from the internal surface of the ciliary ligament to blend with those (circu-

lar ones) which cover the posterior wall of the canal of Schlemm. These facts being ascertained, we can well understand how the immobility of the iris, produced by dilatation of the pupil from artificial mydriasis, produces a condition analogous to the one induced by surgical interference by Graefe in iridectomy, of course in a minor degree.

Having premised the foregoing remarks, it is thought proper to present some cases illustrative of the beneficial effects of mydriasis in corneal disease.

1. E. P., a strumous child, aged three years and five months, was presented for the writer's treatment, in Montreal, Canada, and an examination revealed haziness of the cornea, with two or three minute ulcers near the centre. There was some slight sluggishness about the iris, with no discoloration. The aqueous humor seemed perfectly clear, but there was marked injection about the sclerotica, producing a decided circum-corneal zone; the tongue was dirty, skin pale and flabby, slight fever, and diarrhœa. There was also marked photophobia. Quinia, with hydrarg, cum creta were prescribed, and a collyrium of atropine, one grain to the ounce of rose water. On the second day the diarrhœa had ceased, the tongue was somewhat cleaner, and there was also a diminution of the febrile symptoms; the iris, however, was more sluggish, and the ulcerations upon the cornea changed from the transparent or *punctata* condition to the *disseminata* form, by the giving away of the walls of the ulcers. The quinia was continued, and the atropine instilled every half hour. On the third day there was an aggravation of the ophthalmic symptoms, when the collyrium was strengthened by the addition of another grain of the atropine, and instillations ordered every half hour. On the morning of the fourth day the pupil was widely dilated, but not to its fullest extent; and the haziness, towards the circumference of the cornea, disappearing. A blister was applied to the right temporal region, and dressed with belladonna ointment. On the fifth day there was an amelioration of all of the symptoms, and the instillations ordered every two hours, instead of every half hour. The treatment was thus continued, when, on

the eighth day, the quinia was discontinued. From that period until the fifteenth day the collyrium was used three times a day, the patient taking, in the meantime, plenty of open air exercise, generous diet, and port wine in small quantities. On the sixteenth day all traces of the lesion had disappeared, and the treatment discontinued. Six weeks after, however, from some inappreciable cause, the left eye was also attacked, and in a most violent manner. The same treatment was used, but with the atropine collyrium much stronger, *four grains* to the ounce of rose water. In this instance the disease yielded rapidly, a cure taking place within a period of thirteen days. For nearly a year there was no further attack of the eyes, but the general health of the patient declined, and she became very cachectic, the glands about the submaxillary and axillary regions engorged, and there was constant tendency to diarrhœa or dysentery. The usual antistrumous treatment was persisted in, but as there was no amelioration, a removal to St. Louis was advised. Two months after the arrival in this city, the right eye was again attacked, and the medical gentleman in attendance consulted an oculist, who continued in attendance for three or four weeks, and who used all the proper appliances, atropine included, but in small quantities. The writer was consulted by letter, and the condition as described was of the most aggravated character, the cornea being pearly in appearance, the iris almost invisible, and the prognosis very unfavorable. A telegraphic dispatch advised the use of a collyrium of *eight grains* of atropine to the ounce, on the supposition that there was extensive adhesions of the iris, either to the capsule of the lens, or to the membrane of Descemet anteriorly, and that such enormous applications of the mydriatic would break up the adhesions. The result justified the means, and a rapid cure ensued. In this instance the instillations were made an interval of one hour each.

Case 2. On the 19th of October, 1865, the writer was asked by a physician in this city to see a case of *ophthalmia neonatorum*, which had been under his care for about ten days, and which had up to that time baffled all efforts towards cure. It is

proper to state, that, in this instance, the child was attacked on the second day after birth; and had been entrusted to the matronly care of a midwife for nearly two weeks, and whose treatment consisted in the application of certain herbs and salves, known only to those versed in female obstetrical lore, and initiated into the mysteries of woman's hereditary handicraft. On the fifteenth or sixteenth day, Dr. —— saw the little sufferer and found a most pitiable condition of affairs. The case had long since passed into its second stage, and the phlogosis was at its height. There was great tumefaction of the lids, the discoloration was almost ecchymotic, the free border of the upper ones overlapping the under ones, so much so as to completely cover nearly one fourth of them; the conjunctiva was scarlet in color and chemotic, the cornea opaque, and the purulent discharge welling out of the palpebral fissures and coursing down the cheeks. Applications such as are usually applied were constantly and freely used for ten or twelve days, and the only advantage obtained was a diminution of the secretion; at this period the writer was requested to see the case in consultation. After a careful examination by means of the palpebral elevators, and thoroughly washing the globe with tepid water, the discovered condition of the eyes was about as above described, with the addition of a large ulcer on each. In the right eye, the ulcerated surface was about the size of the head of an ordinary tack, the internal margin of which occupied about one third of the inferior external margin of the pupil; the ulceration had penetrated the entire thickness of the cornea, and there was prolapse of the iris (hernia iridis), a reduction of which was attempted, but failed, lest laceration of the tissue of iris should take place, owing to the extensive (and probably old) adhesions. In the left eye, the ulcer had not penetrated the entire thickness, and occupied a position below the pupil, its superior margin covering the lower fifth of the disc of the iris. The writer proposed a local application of the stick nitrate of silver to the palpebral and ocular conjunctiva, according to the plan of Desmarres, to be washed immediately with a solution of common

salt, the chemical reaction ensuing producing the insoluble chloride of silver; this did not meet with the approbation of Dr. ——, when it was agreed to use a collyrium of twenty grains of caustic to the ounce of water, which was twice daily applied by the writer in person for several days; in the meantime, the mother instilling every hour a few drops of a collyrium composed of *four* grains of atropine to the ounce of rose-water (of course, the purulent secretion being washed away before each application, by means of a camel's hair pencil and tepid water); to the lids, cold slippery-elm poultices, mixed with glycerine, were constantly applied. After the first six days, when the conjunctival inflammation had so much subsided that the discharge was very slight, watery in its nature and yellowish in color, the nitrate of silver collyrium was withdrawn, and another one of sulphate of zinc and morphine, each three grains to the ounce, substituted. After about two weeks, the ulcer in the left eye was totally healed, and the hernia iridis in the right one reduced to the size of a pin's head, the constant application of atropine having almost entirely broken up the adhesions. Five weeks after this plan of treatment was commenced, both cornea became perfectly clear, save where was the the ulceration, the left one presenting then, as it does now, January 11th, 1866, a very slight albugineous character; in the right one, the opaque condition is almost imperceptible, save where there is attachment of the iris. In the left eye the pupillary contractions and dilatations are perfect, in the right eye almost so; there can be no doubt but that the deformity here will, in time, almost entirely disappear.

ERYSIPELAS OF THE FAUCES.

BY J. K. BAUDUY, M. D.

A case of unusual and peculiar interest having elicited my attention a few months since, I have, after some hesitation, determined to report it to the Society, in order to ascertain whether any of its members have been unfortunate enough to meet with cases bearing some analogy with the one in question. I am, moreover, particularly desirous to profit from the experience of others who have had richer and more extensive fields to reap in than myself, for I am at a loss to account for the remarkably slight connection which existed between the symptoms during life and the well marked pathological phenomena discovered after death.

H. F., aged 30, was admitted into St. Vincent's Institution for the Insane, in the early part of July, 1864. He was laboring at the time under acute mania, and had had two attacks previous to his admission, in which he was reported as being violent, destructive and dangerous. Under the treatment of the late Dr. Allen he soon recovered, left the Institution, and obtained a situation. A month afterwards, however, he returned, and remained an inmate up to the period of his death, in April last. The patient, for three months previous to that event, had manifested no symptoms of insanity, and was anticipating a final departure from the Institution. He was a man of vigorous and robust health, and was in the habit of making himself generally useful about the house. All medical treatment had been discontinued some time previous to the commencement of the present year. One Saturday afternoon, in passing through the wards, he accosted me and directed my attention to the fact of his having "slight sore throat," which seemed, however, to cause him little uneasiness or annoyance. Prescribing a simple gargle, I passed on. The next day I was prevented by indisposition from paying

my accustomed visit, so can therefore give no further details of his case from personal observation, but will substitute those of a faithful, experienced and intelligent nurse, whose statements can be most implicitly relied upon. During the course of that day the patient went about his usual avocations, ate a hearty breakfast, and seemed cheerful. Dinner time arrived and found his appetite not only good, but unusually sharpened. In the afternoon he seemed to be perfectly natural, save occasionally complaining of a stiff neck, slight soreness of the throat, and some hoarseness.

There was, however, no fever or dyspnœa, and but very slight dysphagia. The afternoon passed as the rest of the day, and in the evening the patient seated himself at the supper table, little imagining that he was about to take his last repast. Having performed his duty creditably at *this* meal, and the weather being fine, he, with the other male patients, went out to enjoy an evening walk through the grounds of the Institution. Seemingly very calm and natural in his demeanor, he enjoyed this pastime for an hour or more, and then retired to his room, complaining of increased hoarseness and further annoyance from the throat, accompanied with more or less dysphagia.

Having called the nurse, he requested that some application should be made to his throat. His wishes were immediately complied with, but finding no relief to the disagreeable sensations which he experienced, the attendant determined to send at once for me, and left his room for that purpose. He had hardly been absent five minutes before he heard the most unusual sounds proceeding from the patient's room, which induced him to hasten thither in order to ascertain the cause. On opening his door a ghastly and truly dreadful sight was presented to his view. The patient was stretched upon the floor, with features fearfully contorted, and visage blue as indigo—*a corpse*. The death was evidently occasioned by some obscure cause, resulting in *apnœa*.

What was *the cause* of the apnœa I was then unable to explain. Could it have followed the rapid development of cynanche tonsillaris? This was soon determined *negatively* by an

inspection of the fauces. My next conjecture was as to the probability of the existence of a retro-pharyngeal abscess, which, suddenly bursting, might have induced suffocation, as in a case I recollect reading of, in which death was occasioned in the following manner: Corvisart, having occasion to visit his wards rather unexpectedly, so alarmed the attendant, who did not anticipate this unlooked for arrival, that he suppressed an inclination to vomit a hearty supper he had just partaken of, and fell dead on the floor. On inspecting his body soon afterwards, the larynx, trachea and bronchial tubes, were found filled with half digested food.

The contents of a retro-pharyngeal abscess might, likewise, by suddenly bursting and emptying themselves in the larynx and trachea, cause instantaneous death.

But taking into consideration the absence of vertebral disease, and such symptoms as dysphagia, dyspnœa and tumefaction, I was compelled to abandon this hypothesis and seek a more reasonable one. The history of the case plainly pointed to the larynx as the region to be explored, and I was not long in solving the problem as to the actual cause of death. A post mortem examination, made eighteen hours afterwards by Dr. Gregory, assisted by Dr. Brokaw and myself, revealed the existence of extensive œdema of the glottis and epiglottis, with a peculiar appearance about the lips of the former, which struck us as being due to an apoplectic condition, if I may so speak, of the laryngeal mucous membrane; or rather, an ecchymosis, resulting in the formation of minute clots, dotting the membrane here and there, small in size and few in number. The largest did not exceed the dimensions of a small pin's head. There was no abrasion of the epithelial surface of the membrane itself, and no other evidence of inflammatory action could any where be discovered. The parts otherwise appeared healthy, and not the slightest trace of ulceration was discovered.

The œdema of the glottis, unquestionably, caused the patient's death. *But whence came the œdema itself?* The patient had been so singularly robust, and his previous symptoms so

trivial, that I confess my interest to understand and explain to myself the phenomena presented in his case became all-absorbing. How was it possible that such extensive œdema of the glottis should supervene so suddenly and result so fatally, and yet manifest *no* alarming or urgent symptoms whatsoever during life? It is most distressing to know that a malady so antagonistic to life can exist so insidiously and end so fatally, without our even having a suspicion of its presence, or of the rapidity with which its ravages are being effected. Acute laryingitis, as ordinarily met, was out of the question, as its characteristic accompaniments of high fever, frightful dyspnœa, harsh and painful cough, were all absent. It is true that œdema of the glottis sometimes occurs without preceding inflammation; but such cases are comparatively rare, and generally result from pressure, retarded venous circulation, tumors, aneurisms and chronic laryngeal affections. Hence all these causes can be cast aside when we take into consideration the previous history of the case. I had no remorse as to neglected treatment, for I felt satisfied that, had it been possible to arrive at a correct diagnosis, it would have been attained too late to have given time for the successful performance of tracheotomy, so rapid was the apnoea. Had I been on the spot, the only procedure possible, with any hope of success, would have been the one described and recommended by Dr. Watson in his lecture on acute laryngitis. I refer to the penetration of the larynx with a small curved trocar, the place of selection being the membranous space between the thyreoid and cricoid cartilages. For four or five months I have studied over this case with a view of explaining the supervention of this œdema of the glottis, and like many puzzles which are apt to have a sudden solution, it has, at least in my mind, been satisfactorily accounted for; though I do not dare to flatter myself that all here present will sanction my convictions. Not long since, I rather unexpectedly called to mind the description I had read in the work of the late Dr. Todd on "Acute Diseases" of a peculiar affection, which I believe he was one of the first to describe, and to which he gave the name of "Erysipelas of the

Fauces." Watson, Budd and various other authors, speak of the unquestionable tendency of erysipelas, and the other exanthemata, to affect the throat, and thence extend to the glottis, sometimes producing sudden death. Yet I believe Dr. Todd properly claimed the right of priority in describing the affection just alluded to, as no systematic writers that I am acquainted with mention it in their works. My idea, therefore, of the case is as follows: Erysipelas of the fauces, or some analogous affection occurred; extension of the inflammation to the laryngeal mucous membrane followed, causing œdema of the glottis, which, in its turn, resulted in death by apnœa. There are other reasons upon which I base these conclusions, which I shall enumerate immediately, only digressing for a moment to remark that the *non*-appearance of the erysipelatous inflammation on the skin does not necessarily invalidate the diagnosis, as the weighty authority of Watson goes to prove that the disease may entirely spend itself on the throat. In referring to this subject, he alludes to the observations of Dr. Budd, who, in a paper read before the Medical and Chirurgical Society, in 1847, relates several cases, and refers to others, which justify the belief that acute *œdematous inflammation of the larynx* is connected more frequently than had hitherto been supposed, with *erysipelas*. That disorder is apt to be propagated from person to person by infection; and the effects of the poison, which thus excites the disease, by contaminating the blood, are sometimes displayed first of all in the fauces and their vicinity. Now, these effects, redness and thickening of the epiglottis, and of the lips of the glottis, with the effusion of seropurulent fluid in the submucous areolar tissue, sometimes prove fatal, producing death *by apnœa* before the erysipelatous inflammation has *had time* to spread far or declare its true character by developing itself upon any part of the external skin.

In concluding, therefore, I will only state this very remarkable coincidence, which only came to my recollection within the last few days, and has a decided bearing upon the case, by adding additional testimony in favor of the conclusions at which I have arrived. I refer to the existence of a case of erysipelas in the

same ward, in which the patient whose case I have just related, was. An old gentleman, past the meridian of life, had an attack of idiopathic erysipelas of the face, and during his sickness was visited by our patient. I doubt not but that some connection existed between the outbreak of the disease in the former and the sudden death of the latter. It is true that the interval between the two occurrences referred to was *little* over a month; but it must be borne in mind that, although many admit the contagious properties of erysipelas, few have any definite ideas in reference to its exact period of incubation. At all events, to me the lesson was a most instructive and interesting one, and in future I shall pay strict attention to all complaints (no matter how trivial) referred to the region of the throat, as even many of the ordinary so-called "sore throats" of the winter season are, I believe, in many instances but mild forms of acute laryngitis, connected with inflammation of the fauces, which, neglected, may be the precursors of the most insiduous, dangerous and fatal affection.

CONJUNCTIVITIS.

BY JNO. W. TRUDER, M. D.

I mean by this term to include inflammation of the mucous membrane of the eye, and something more.

The form of conjunctivitis that I design calling attention to is that variety that usually presents itself as an epidemic. The mucous membrane seems to be the principal membrane involved, yet there can be no doubt but there exists disease of the sclerotic also; and hence we have combined with what is usually termed *catarrhal*, a *rheumatic* affection of the eye.

The form of opthalmia that committed such ravages in the French army during its sojourn in Egypt, and also the English army, under Sir Ralph Ambercrombie, is set down as purulent opthalmia, and was considered contagious, or capable of being conveyed from one individual to another by actual contact. The form usually witnessed in this country is in some respects similar, from its principal seat being the mucous coat, but on account of its lacking that excessive purulent discharge and rapid disorganization of the structure of the eye, does not arise to the importance of Egytian opthalmia.

It is not always an easy matter—in fact, I have found it at times very difficult—to arrive at a correct diagnosis of diseases of the eye. For, while from external appearances we are led to believe there is nothing more than inflammation of the mucous, there may be a disorganizing inflammation at work in the sclerotic coat. We are not at a loss to distinguish the existing turgescence of the sclerotic, so long as the mucous membrane is but partially involved; but when this membrane, from its excessive vascularity closes out the sclerotic from view, it becomes difficult to determine to what extent that membrane is implicated. The diagnosis is not only rendered uncertain from ecchymosis of the conjunctiva, but is almost impossible, also, from purulent infiltration between these membranes.

The fact I wish to impress is, that the conjunctivitis so common in this country, when it takes on the character of an epidemic, is not only an inflammation of the conjunctiva, but of the sclerotica also, and depends in a great degree upon miasmatic influence, as well as direct cold applied to the surface. While a simple conjunctivitis, the result of cold only, will yield readily to mild apperients and collyria, the form to which we refer is influenced but very little, either pro or con, by this mode of treatment. We cannot style this affection either strumous or purulent, from the fact that it is wanting in a very prominent particular—that of excessive purulent discharge and rapid disorganization of the structure of the eye. We propose to call this affection of the eye, in order to be better understood, *miasmatic*,

from the fact that it seems influenced by miasmatical causes, and partakes of the periodical type of disease.

We will here give a short clinic that was made over a year ago. We give these cases in extenuation of the plan of treatment then adopted, and not that we expect any new ideas advanced in regard to the pathological history of conjunctivitis; for we do not claim anything new in regard to the disease, its symptoms, etc. The cases here presented were all reported to "Medical Department U. S. Army," in "Report of Sick and Wounded" for the month of December, 1864, and are as follows:

1. James Barnes, private, Co. E, 1st Cav., M. S. M., admitted December 4th, 1864, disease conjunctivitis, was placed upon collyria of cupri: sul., which was continued, together with various other general and local treatment, without any apparent benefit, until the 21st of said month, when the following prescription was ordered:

R. Quinia sul.....gr. iij.
Morphia sul.....gr. ¼,

given every three hours. I will here state that this soldier had suffered from periodical attacks of conjunctivitis all summer and fall, which resisted the usual course of treatment, and only received permanent relief after the quinine treatment was adopted.

2. F. W. Shriver, private, Co. G., 1st. M. S. M., admitted December 6th, 1864, disease conjunctivitis, was placed under the usual treatment, receiving permanent relief only when the quinine treatment was adopted.

Cases 3, 4 and 5 were admitted during the month of December, and all received immediate relief from the quinine and morphia treatment.

I will here append the remarks accompanying my Report of Sick and Wounded, for the month of December, 1864:

"Before closing my remarks for the present month, I wish to submit to the Medical Department a plan of treatment that I have found most efficient in conjunctivitis. After using the prescribed treatment in such cases, with uncertain results and a vast amount of suffering to the patient, I came to the conclusion that this in-

flammation depended upon, and was in a great degree amenable to, local miasm, and was, in fact, a derangement of the whole system. I accordingly prescribed quinine, in three grain doses, combined with a quarter grain of sul. morphia, or one grain opii, every three hours. This was given in the day time, and the patient allowed to rest at night. In the space of ten days, the worst cases of conjunctivitis was radically cured.

"I am satisfied that the treatment would have been more speedy had the patient been treated in hospital, where some prescribed diet could have been used.

"Four of these cases were very bad. The conjunctiva was very much swollen, congested and red, lying in folds upon the cornea. Excessive photophobia was also present, and the eyes were continually flooded with tears. No local application was used in any of these cases, except a solution of chloride of sodium and a simple lead wash alternated."

I do not remember of ever noticing this plan of treatment recommended, and would respectfully ask a liberal consideration from the profession. My experience in it being limited, I am not prepared to say that it is infallible; yet I am certain that I have never used a remedy in this very obstinate disease that acted with such promptness, or was half so efficient.

Correspondence.

SYPHILITIC MALADY.

A collection of Notes on the origin of Syphilis, the different Doctrines, &c., maintained by the different European Authors—embracing a few questions to M. Ricord, with his Reply, &c., &c.

[Continued.]

BY DR. A. C. HAYNES.

DUALITY.

From this theory the infecting and the simple chancre have, in appearance and position, some relation; but they constitute two morbid states, completely different. In 1789, Henseler had already developed the idea of dualism; he pretended that the simple chancre was known to the ancients, and that syphilis, on the contrary, is a new disease. Jaudt, in 1834, set forth the same opinion. The real author, however, of this doctrine, is the learned Bassereau, who has discussed it at length in one of his works, which is one of the most remarkable that has been written upon syphilis. He says:

1. "The simple chancre (ou ulcere contagieus des organes genetaux, non suivi d'infection constitutionnelle) was known to the ancients."

2. "Syphilis, and of course the infecting chancre, which is the primitive manifestation of this disease, are of a modern origin; they were imported into Europe after the discovery of America."

We hope that we will be able to prove that the diseases transmitted by sexual intercourse, of which the ancient authors speak so much, were nothing else than the simple chancre. Celse has described with care the different ulcerations of the genital organs, and his authority is quoted by all partisans of the ancient origin of syphilis. He says: "When followed by inflammation, the prepuce is swollen in such a manner that the glands cannot be uncovered or covered again: it is necessary to apply repeated fomentations, with warm water: if the glands are exposed it is necessary to inject it between the gland and prepuce. When the prepuce has been drawn back, ulcers situated on the glands can be perceived. These ulcers are clean and dry, or they are very humid and purulent. Sometimes the penis is so greatly ulcerated through the prepuce by these ulcers that the glands slough."

Farther on, page 376, he says:

"Sometimes a kind of chancrous ulcer forms on the penis, to which the Greeks gave the name of phagedenic. This evil demands a prompt relief, and can be treated in the same manner as the others: if the remedies have no effect it will be necessary to employ the actual cantery."

Has any person ever seen an infecting chancre destroy the glands and prepuce? Is phagedænia a complication ordinary of simple chancres? Do we not find, on the contrary, in well declared symptoms, all the characteristic traces of simple chancre? How, then, does it happen that Celse, who has devoted several pages to the different affections of the genital organs, did not say a word of their terrible *consequences*, were he acquainted with them? Especially is his silence on the subject a matter of astonishment, if syphilis existed in Europe at the time he wrote his book.

Hippocrates has spoken of bubo in the groin produced from the ulcers of the nubin and of the vulva; but is not bubo the most common complication of a simple chancre? Orisbase, D'Aetius, Marcellus l'empirique, Paul D'Egine, all speak of ulcers of the anus, of the vulva, and of the glands giving rise to a serosanguinolent secretion.

To be impartial, we should refer to the arguments upon which those supporting the ancient origin of syphilis rely, and commence by citing the malady of Job, which some have supposed was syphilitic on account of the crust which covered his body, and the ulcers with which it was infested. The Leviticus says that he was affected with lepra; does not the lepra explain his condition much better than syphilis? Aretas speaks of an individual whose "uvula was destroyed as far as the palate-bone, and whose fauces were destroyed as far as the roots of the tongue and the epiglottis; after this destruction the patient could no longer take drinks or solid food, and he died from inanition."

We have searched with ardor into these collections, and have extracted from them some quotations that we have found in all the works upon syphilis.

In the history of syphilis, written by Rosembaume, who was one of the most tolerant, erudite and diffuse writers, opposed to the doctrine of duality, we will find a strope, taken from a collection of verses to Priapus:

Cum penis mihi forte læsus esset
Chirurgique manus, miser timerum,
Diis me legitimis, nimisque magnis
Ut Phœbo puta, filio que Phœbi,
Curatum dare mentulam verebar.
Huic dixi; fer opem, Priape, parti,
Cujus tu, pater, ipse par videris.
Quâ salvâ sine sectione factâ
Ponetur tib picta, quam levaris,
Par que, consimilis que, concolor que.

The author of these verses had his genital parts affected with a disease which necessitated surgical interference; he addressed them to Priapus to ask his intervention. Where can we find in this quotation a word which is significant of constitutional contamination?

Will we succeed in finding proof of the existence of syphilis in Rome from the following epigram of Martial, in which he alludes to the fact of an entire family becoming affected with ulcers of an equivocal nature?

Ficosa est uxor, ficosus et ipse maritus
Filia ficosa est et gener, atque nepos.

Martial, in another of his epigrams, speaks of a libertine who had communicated to a girl, with whom he had an impure connection, an ulcer of a contagious nature:

Mentula quum doleat puero; tibi, Nœvole, culus
Non sum divinus, sed scio quid facias.

The same poet, speaking of a certain Baccarian, writes:

Curandum penem commisit Baccara Græcus
Rivali medico; Baccara gallus erit.

Zonare states that the Emperor Golere Maximien had an ulcer which destroyed the greater part of his genital organs; the same fact is reported by Eusebe de Pamphylia. All these instances cited have reference to local lesions only; this is an undeniable fact.

The bishop Palladius relates that a person named Héron, went to Alexandria, where, "urged by the devils, he frequented bad places; in this manner, he fell into the mire and lewdness; he contracted relations with a comedian, and showed to him the girdle (disease.) After he had accomplished this act, by the Divine will, a carbuncle formed upon the glands, and he was so sick during six months that his genital parts became rotten and sloughed. Being cured, after a while, he returned to the knowledge of God." It is very evident that in this case a simple chancre is referred to. This ulceration has the greatest resemblance to a suppurating anthrax, and it is not astonishing that the good bishop should have chosen this word to express the nature of the lesion.

We know, moreover, that anthrax is not contagious, and that its duration is six months; these two characteristic symptoms correspond exactly with a simple chancre, resulting in a phagedenicaction which destroys the penis and heals without having caused constitutional infection.

Some have endeavored to prove the existence of secondary symptoms resulting from syphilis in the Romans, from some passages of Martial and of Persius, referring to the hoarse voices

of the Cinedes. Is it not easier to explain this fact by debauchery and drunkenness, which, even in our own days, imprints this stigma upon infants of the lower classes, and especially is to be remarked in those living on the barriers and fauborgs of Paris?

All doctors of the middle age have described contagious ulcers of the genital organs, but none of them have said a word which is suggestive of syphilis.

Some have made great efforts to prove the ancient existence of syphilis from the rules established in England and France, during the middle ages, by the medical police regulations concerning public women. They have especially cited the fourth article of the statute, published in 1347, by Joan I., Queen of the Two Sicilies, comtesse de Provence, concerning the house of debauchery at Avignon. Contagious maladies of the genital organs were mentioned as in all the other instances already cited; but no intimation was given that the ulcers spoken of gave place to general infection. This article is thus known: "The queen wished that, every Saturday, the baliff and a surgeon, appointed by the consuls, should visit each courtesan, and if any one was found to have contracted disease, arising from lewdness, she was to be separated from the others, because, living apart, she could not hold connection with men."

[Conclusion in the next issue.]

TRICHINA SPIRALIS.

BERLIN, PRUSSIA, January 15th, 1866.

MESSRS. EDITORS:—The subject of *Trichina Spiralis* has not, doubtless, attracted the same attention with you as here, and some account of the animal may not be devoid of interest to your numerous readers. I have therefore collected together some of the most important points, chiefly from reports and pamphlets published in regard to these interesting little animals, which, if you think of sufficient import, you can lay before your readers.

Here, the subject has attracted the attention not only of the profession, but of the people generally. Passengers on the railroad and steamboats talk of it; newspapers have articles written on it, and from the interest taken, a mass of information has been obtained—some would think quite disproportioned to the importance the little animals bear to mankind; but as man is affected through his food, any knowledge which will prevent his infection will not only be interesting, but useful.

The trichina is a parasite found in carnivorous and omnivorous animals. It has been seen in rats, mice, carnivorous birds, swine and human beings. It has especially attracted attention from its existence in the swine, for by this channel it enters man. The swine is the ante-room, where it waits to be received into the human temple, there to work its destructive changes. In 1830, or thereabouts, a Mr. Hilton recognized small white points in the human muscles, but failed to perceive their true nature. This remained unknown until 1835, when Mr. Owen determined, by means of the microscope, that these white points contained small worms, rolled up in a spiral form. By him they were named *trichina spiralis*: *trichina*, from their fine, hair-like form (triches), and *spiralis* from their rolled up condition. The trichina is truly and properly a worm, and though it often attains the length of a third or a half of a line, which the naked eye could readily see, yet it is so transparent that the unaided vision cannot take cognizance of it, unless it is rolled up in a very compact mass. If the animal into which it is introduced lives long enough it will become encysted, and in the course of time the cyst becomes calcified, and then may be apparent to immediate observation. When this takes place all danger is past, and it was owing to the non-observance of the trichinæ until nature had thus established a cure, that they were regarded as harmless to the human subject.

The early observers were in total darkness in regard to the origin and history of the trichina. They neither know whence it came nor how it was propagated. To help themselves out of the difficulty, they advanced the theory of spontaneous generation.

This was the theory in regard to all intestinal worms, the more plausible concerning the trichinæ, as they appeared in such almost limitless numbers. Spontaneous generation is now viewed as a false theory, and though there is yet much to be learned, nevertheless the profession is settled in regard to many important particulars concerning the origin, propagation and influence of the various entozoa. Trichinæ have been divided into two classes, muscular and intestinal. Experiments have been made by feeding animals with the encysted muscular parasites, and in the course of three or four days, perfectly organized worms of both sexes have been observed in their intestinal canals. The muscular trichina is the young, imperfectly developed state of the animal, which in the intestines attains its full development. From these latter, young are born, which migrate into different portions of the body. The limit of their migrations is the primitive fibrilla of the muscles, and it is here especially that they make themselves a dwelling place. The disturbing influence is exercised mostly on the muscular substance. The fibrilla disappear in most of the fibres, and the latter are in that degree emaciated. The irritative influence, however, is seen on the sarcolemma, especially at that point where the animal remains any length of time. This becomes thicker, the muscular corpuscles enlarge, and a more solid substance is deposited between them. The larger the animal grows the more is it rolled in a spiral form, and at that point where it comes in contact with the sarcolemma, the cyst is less transparent, even from the beginning. If we look at such muscles with the naked eye we can scarcely observe anything peculiar, and the microscope is needed in order to demonstrate their actual condition. The cyst remains transparent for a greater or less length of time, but finally begins to calcify. This calcification proceeds gradually, and for some time the worm can be seen in the centre in its spiral form, but it is eventually entirely enclosed, and we see nothing but a white point.

There has been observed in the swine, and in some other animals, a white object, which by some has been mistaken for encysted trichinæ. But closer observation has shown that this is a different

object. When pressed under the microscope a number of starch like grains have been seen to issue, which are entirely foreign to the trichina cyst. Then if sulphuric, or acetic acid be added to the trichina cyst, it will vanish. This has no influence, however, on the bodies above mentioned. It should not, however, remain unnoticed that this test for these calcified cysts has more a positive than a negative value. If the white point vanishes on the application of the acid, we may safely infer that it was a calcified cyst. If, on the other hand, it remains visible, we cannot certainly draw the conclusion that no trichinæ are present; for the cyst may be partly apparent from the presence of fatty globules, which are uninfluenced by the acid. We have said that the trichinæ in their migrations lodge finally in the muscles. They are seldom found in the glands, such as the liver, the spleen, the kidneys, or in the skin, but seem to have a preference for the muscles themselves. They wander as far as they can conveniently: when they meet with any obstacle to their progress they stop. Such an obstacle they have in the tendons of the muscles. It is a remarkable fact that though all other muscles are liable to become infected, the heart alone remains intact. They have never yet been discovered in this organ. Though they are chiefly found in the muscles, yet not solely; they have also in rare cases been seen in the peritoneum, in the pleura, in the pericardium. This discovery answers the interesting and important question, how do they reach the muscles? According to what we have just seen they must make their journey by means of the solids. The closest observation has never been able at any time to detect them in the blood.

Thus far we have been occupied with the encysted trichinæ. In order to see the worm independent of its sack, we must make use of a microscope of a very high power. The experienced eye, it is true, can make out a white point under favorable circumstances, that is, when it is rolled up in a compact mass; but it is impossible for any one to say that this point is an animal. Its motions are very limited, and are confined to an enlargement and diminution of the spiral. If it stretches itself, it becomes invisible on account of its transparency. If we bring the animal under

a glass, which magnifies as many as three hundred diameters, a perfectly well organized worm is observed, having alimentary canal and organs of generation in a good state of development. If a weaker glass is used, we can make out scarcely anything more than a bare outline. So much for the general discussion. Let us pass now to a more minute consideration of the operation of these parasites on the human body. Extensive experiments have been made by Prof. Virchow on inferior animals. Rabbits have been fed with these encysted trichinæ, and they have all died in the course of three or four weeks with manifold symptoms. His observations have led him to the following conclusions. The muscular trichinæ are taken into the stomach, and are there more fully developed. Male and female animals are then first observed. The female is filled with young, which are born in the course of a week after their reception into the alimentary canal. Each one contains as many as one hundred young, and if we consider how many of these little animals may be taken at one mouthful, we can form a faint conception of the immense numbers that are generated in the body. Let us suppose twenty thousand are swallowed at once, which often happens; taking it for granted that the sexes are equally divided, ten thousand of these will each bear one hundred young, making in all one million. These young, then, commence to penetrate the tissues of the body and call forth various symptoms. First, there is generally observed an increased secretion of the mucous membrane of the alimentary canal. Diarrhœa has been present to such a degree that the case has been treated as one of cholera, with the most fatal consequences. Then, there are most always muscular irritability, muscular weakness and rheumatic phenomena present. These symptoms are readily explained, when we consider what an irritation these little borers must cause in their migrations. Finally, symptoms are frequently present which very much resemble those of typhoid fever. This is what the physician sees when called to one infected with these parasites. That death is a consequence of the trichinæ, is made evident by the fact, that post mortem examinations show the tissues saturated with them,

whereas other pathological changes are not discovered. Death in no case takes place before three weeks after the infection, generally in the sixth or seventh week. If the patient dies within five weeks after the trichinæ have been eaten, a typhoid infiltration of the mesenteric glands has always been discovered, unattended, however, by inflammation of the mucous membrane of the intestines. In the fourth week, light streaks can be observed in the muscles even with the naked eye. In the sixth week after infection, lung affections are observed, which the *post mortem* confirms. It is a pneumonia exactly similar to that which follows or accompanies typhoid fever. The blood is found, likewise, to be only slightly coagulable. In the seventh week, liver affections usually make their appearance. Insufficient respiration, owing to the presence of trichinæ in the respiratory muscles, doubtless brings about the lung trouble, as well as the change in the blood; the patient always complaining of difficulty in breathing.

These pathological changes are only brought to pass by large numbers of these little animals. A few can exist in the body without any evil symptoms following, but a large immigration must cause disease and often death. In the last few years, there have been many very fatal epidemics in Germany. In a little town in Saxony, last fall, there was an epidemic, with a mortality of nearly fifty per cent., caused by but one infected swine. The practitioner, at first, treated the patients for cholera, with opium, and doubtless death was brought about in many by this means. The very interesting and inexplicable fact was observed in this epidemic by Dr. Conheim of Berlin, that out of sixty children who took sick, not one died. The question has been advanced, how is it that the disease was not known before? Some call it a new disease, some deny its existence altogether; but the disease is not new, simply the knowledge of its true character. It was once quite common here to speak of *ham-poison*, a substance which has never been isolated. The disease was observed and ascribed to this unknown agent; but all attempts to isolate the poison failed. There is an instance on record where a number of gentlemen breakfasted together, about thirty years ago. One

of the number, being engaged with some business, did not sit down, but simply drank a glass of claret, the rest partaking of raw ham and white wine. The latter all took sick and some died, while the former remained well. Suspicion was fixed on the wine, and it was chemically examined, though nothing discovered. A few years ago one of these gentlemen was operated upon by Prof. Langenbeck, and his muscles were found studded with white points, which proved to be trichinæ. Many similar instances might be mentioned which baffled the skill of the physician to understand or treat, but which at present can be easily explained. Another question will probably occur to the mind of the reader, is any region specially adapted to the trichinæ? Why has the disease been found so rarely in America? Why has it been observed especially in the north of Germany, and not at all in the south? In answering these questions, we must not forget that it is a very rare disease in the swine. At the very highest reckoning not more than one swine in every five thousand is infected in Germany. The disease may have occurred among us without its being recognized as such; for, as we have seen, the symptoms can very readily be mistaken for those of other diseases, but I doubt very much whether there have been many cases of it in America. The true answer to these questions is found in the different mode of preparing the food. In southern Germany, as well as with you, all swine meat is much more thoroughly cooked than here. We shall enter into this matter more fully below.

We come now to inquire, what the doctor has to do in such a case. The forming of the cyst in the muscles is nature's method of cure, but this process cannot be hastened by any means in the Materia Medica. As soon as the cyst is formed, the patient can be pronounced entirely out of danger. The worm has such a low state of life in its narrow house that it gives no inconvenience. Our means must be limited to the trichinæ while yet in the alimentary canal. An emetic is very serviceable, if the infection is discovered before the food has left the stomach. This is, however, rarely the case. The most usual and practical treatment is purging. The object is to remove these animals from the body

as quickly as possible, before the young have left the alimentary tube. This is the remedy that nature herself suggests; for we almost always find vomiting and purging as prominent symptoms in the disease. After the worm has left the intestines, of course, we can accomplish nothing by laxatives. Many drugs have been proposed for the purpose of acting on the trichinæ after they have left the intestines. None have, however, as yet been discovered which are efficient. It has been suggested to me, by an American physician, that the hyposulphites might act favorably, from their known property, as antiseptics. Though, of course, the suggestion is worth being tried, I confess to have little confidence in it. These trichinæ are very tenacious of life. Prof. Virchow mentions that he placed a piece of muscle with unencysted trichinæ in phosphoric acid of such strength, that the muscular substance was so affected that the fibres could be separated with the greatest ease with the fingers; the trichinæ remained alive.

Though no remedy has been found, yet much may be done by way of prophylaxis. Here lies the whole duty in regard to this subject. If the description that we have given of the disease meets with any credence, none can fail to make use of proper preventives. In the first place, much can be done by keeping the swine free from infection. They are doubtless infected by rats and mice. In districts where the disease already exists, it may be more extended among swine by their eating the fæces of such patients. We do not deny that it is a difficult thing to regulate the food of the pig entirely; yet this is a matter which should receive our most serious consideration. An absolute vegetable diet is essential in order to be perfectly sure that no infection has taken place. There is the more need of this because there are no symptoms which will indicate to us that a certain swine is infected. The young pig is most affected; in it we often see symptoms which correspond in some respects to those of the human being, yet in neither old or young are the symptoms at all characteristic. The very same phenomena may be observed in an entirely different condition than that of infec-

tion with trichinæ. The purchaser of a swine can never know if it is free or infected.

Another prophylactic measure is the microscopic examination of the pork. This is very extensively done in Germany. The practical application of this plan, however, has many difficulties. which need not be described. Let us take for granted that such an examination is faithfully and minutely carried out, yet at the best we can only gain a probability. We are not able, from a microscopic examination, to say that no trichinæ are present in the examined swine; the most we can say is, we have found none. It is absurd to talk about examining the whole animal; the greater portion remains uninspected. These parasites are not equally distributed through all muscles; in some parts there may be more than in others; in some, none at all. It is always possible that just those parts may be examined where there are only a few, or even none. This method has a positive value in establishing the presence of trichinæ, but less a negative value.

The surest and best means of avoiding the disease is found in the manner of preparing the food. In many places here it is customary to eat raw pork, finely hacked and spread on bread. Where this custom has existed, the worst epidemics have prevailed. These cases are very characteristic: some members of a family have eaten of the raw fresh pork, others of the same swine cooked. The former have been violently attacked, whereas the latter have escaped altogether, or been but slightly sick. Every degree of cooking is not sufficient: to kill the trichinæ a heat of 60° Reaumur, 192° Fahrenheit is required. If the food is cooked so that it attains this temperature *throughout*, we may eat of it with perfect impunity. But it very often happens that when a large roast is prepared, the surface is thoroughly cooked, while the interior remains below this temperature. Pork which has been strongly salted for a length of time is less dangerous than the fresh meat. The smoking of hams does not necessarily destroy the lives of the trichinæ. What is called *cold smoking*, where the temperature remains low, does not kill them.

Very truly yours,

OSCAR W. GAUSS.

CENTRAL P. O., ST. LOUIS COUNTY,
January 15th, 1866.

MR. EDITOR:—In the last number of your excellent Journal my attention was attracted to a monograph by Dr. W. A. Shaw, upon the subject of cholera, the views embraced in which, commend themselves so strongly to my judgment, that I desire to return my thanks to the author for his valuable contribution to medical science. He very truly remarks, that there are indications of the existence of a panic among the people in reference to the approach of this much dreaded visitant, which it is the duty of the medical profession to allay by every means within their power, and especially. by the inculcation of correct pathological and therapeutical views in reference to the disease. But whilst the common people seem to be appalled by the proximity of the Asiatic monster, the profession look on with apathetic indifference, and devote their valuable time and talents to the investigation of such abstruse and complicated subjects, as the unal or dual nature of syphilitic virus, and others of a kindred character, which, however interesting they may be as ingenious speculations, are certainly greatly inferior in importance at this juncture, to a proper knowledge of a disease with which we will in all probability be called upon to grapple, and which heretofore has very justly been regarded as the "opprobrium medicorum." Perhaps, however, they regard the disease as one, the causes of which are involved in such obscurity, its pathology and therapeutics in such uncertainty, that it is a waste of time to attempt its investigation. But I respectfully suggest, that the difficulties of the subject only increase their obligations to prosecute its study. To the faithful soldier, obstacles, so far from appalling, serve as additional stimuli to renewed exertions to overcome them. But I began this communication as a resume of Dr. S.'s valuable contribution, the most valuable, as I think, which has made its appearance in the pages of the *Journal* during the past year. It is, I conceive, a movement in the right direction, so far as the etiology and treatment of cholera are concerned. I am very

much inclined to concur with his views, attributing the disease to the same causes which produce pernicious or congestive chills. I, years since, marked the striking resemblance in the diseases, or rather in the symptoms, for if the views of Dr. S. are correct, they are the same, differing only in degree, and in the more malignant variety of type, modified or rather intensified by climatic vicissitudes and other physical causes. I remarked to friends that I could extemporize a very fair case of cholera by uniting a "congestive chill" and "serous diarrhœa." So far as my knowledge extends, to Dr. S. belongs the credit of being the first boldly to assume the position that they were identical. A view very strongly supported by the symptoms usually present in the two classes of cases, viz.: 1st, intense capillary congestion; 2d, hippocratic countenance; 3d, feeble thready pulse; 4th, labored respiration; 5th, icy coldness of the extremities, the surface, and even of the tongue and breath; 6th, imperfect oxygenation of the blood from which arises the expressive term "asphyxia," as applied to cholera; 7th, severe and most distressing spasms or cramps usually, although not invariably, present. The presumption of identity of origin is greatly strengthened in view of the fact, that, if relieved at all, it must be by the timely administration and application of stimulants internally and externally. In other words, reaction must be brought about or the patient must die. I desire, however, in support of these views, to advert briefly to a fact of great significance in this connection, viz.: that the ravages of cholera are exclusively confined either to tropical regions, or to the more heated periods in temperate latitudes. In other words, cold destroys the choleraic poison, or at least suspends its effects. Whilst, on the other hand, diseases of zymotic origin, or those originating in specific animal poisons, as small pox, typhus fever, plague, etc., prevail more extensively in winter, and seem to acquire a greater fatality in a low temperature. It may be asked, of what practical importance is this fact? I answer, much every way. It draws a broad line of distinction between these two classes of diseases, and thus far simplifies the subject. It does even more. It goes very far

towards exploding the contagious theory of cholera. We have yet much to learn in reference to this interesting disease; but I would, with becoming modesty, suggest, that it is more in accordance with correct etiological views to attribute it to a known morbific agent, than to make excursions into the domain of fancy in the vain effort satisfactorily to account for it.

In reference to the treatment of cholera, I am of opinion that in the present state of our knowledge, Dr. S. has well nigh exhausted the subject. Reasoning "a priori" we might very readily conclude, that if he is correct in regard to the causes of the disease, and has succeeded in establishing a fair presumption, that malignant cholera and pernicious or congestive chill are identical, that then the treatment which has proven most successful in the latter would, with proper modifications, be most successful in the former. As before remarked, this view of the case is the one most strongly supported by the experience of the profession. Were I to suggest anything in addition, it would be, where all other means of bringing about reaction had failed, to resort to moxa, or even the actual cautery along the course of the spine, upon the principle, if you please, that desperate cases require desperate remedies.

Hoping that the time is not distant, when malignant cholera may be to a great extent divested of its present appalling character and alarming fatality, and that others better qualified to treat this difficult subject may be induced to give the profession the benefit of their experience, I am with great respect,

Yours,

S. T. BASSETT.

NEW MARKET, PLATTE Co., Mo.,
January 25, 1866.

GENTLEMEN—Believing that you heartily desire the promotion of the cause of medicine, and *knowing* that your *Journal* is now established as the leading exponent of legitimate practice in

the great interior valley of North America, I think it the duty of the profession, embraced in that extended region, to sustain you in your highly laudable enterprise; not only with a liberal patronage in the way of subscriptions, but should make the *Journal* the medium through which to give the world their ideas, should they be possessed of any worthy of a place in its pages. Hoping that these sentiments may elicit your kind concurrence, I will venture to offer you the notes of a case of placenta prævia, that recently came under my charge.

Case. About the 25th of September, 1865, Mr. Jacob D. called to consult me in regard to the condition of his wife, a spare and delicate lady of 28 years of age, who, he informed, me was now at the end of the seventh month in her fourth pregnancy. The cause of solicitude was, that she had been attacked, a few hours previous, with a very profuse flow of her "monthlies," to use his own words. I informed him of the probable nature of the case, made a prescription, enjoined rigid compliance with the hygienic regulations usual under such circumstances, and requested that he should let me hear from her occasionally. I heard nothing more from the case until Sunday morning, November 19, when I was requested to visit her, as her friends supposed her in labor. On my arrival, I found her with slight pains, which had commenced on the previous evening, but no other indications of immediate labor; so I prescribed some doses of morphia, and returned home, with the understanding that I would return immediately upon the occurrence of any urgent symptom—especially should hemorrhage supervene. I had no further news from the case until Friday, November 24, when I was again hastily notified to attend. I found a midwife in attendance; she informed me that she had been with the patient four or five days, that she had had repeated bleedings and irregular efforts at labor from her first arrival. I now examined the patient, and found her in the most pitiable condition,—almost pulseless; face white as marble; extremities cold; bed filled with coagula; the bed clothes and everything about the person completely saturated with blood; in fact, there was all the evidences of extreme prostration common

after exhausting hemorrhages. An examination, per vaginam, showed the blood to be still flowing freely; the os uteri dilated to the bigness of a silver half dollar; the pains recurring at long intervals and very feeble; the placenta centrally located, with the right lateral and anterior lips of the os completely detached from it. I immediately introduced a tampon tightly into the vagina, securing it in place with the usual T bandage and compress, with the view temporarily to arrest the flow, and thereby save the patient from immediate death, and to gain myself time to consider the best measures to be adopted in the future management of the case; not presuming to suppose that by the use of this simple means, I had adopted the happy expedient that was to save life to my patient, and relieve me from one of those unpleasant positions so perilous to the character of the young physician,—a place from which such names as Meigs, Pallen, Bedford, Lee or Simpson might well be pleased to escape. The hemorrhage immediately ceased; the pulse speedily regained its volume; the cheek recovered its usual tint; the pains took on new vigor;—in short, the labor progressed as nicely as if nothing abnormal had happened. This favorable condition continued until it was presumed that the indications gave reason to believe that the dilatation must be well nigh completed; I then administered the secale cornutum freely, until there was evidence of its vigorous action on the womb; the bandage, compress and tampon were then removed, and were immediately followed by a large coagula and a quantity of fluid blood, showing that the hemorrhage would speedily become more troublesome if delivery was not soon accomplished. I at once introduced my hand and found the dilatation complete, with the placenta filling the space usually occupied by the protruded membranes at this stage of labor. I passed my hand through the organ, the fœtal head immediately descended through the rent, and with a pain or two the delivery was completed. The child was dead, and had been so for many hours, from its appearance.

The placenta remained strongly attached, by its left margin, to the walls of the cervix, and I experienced some difficulty in effect-

ing its removal. Nothing of interest occurred to the patient, until within the next three or four days, when she was attacked with a metritis, from which she is not yet entirely recovered. She had had an attack of crural phlebitis, following her third accouchement, for which I treated her. Now, this case offers another example of many that I have noticed, viz: that when there is any abnormal condition present during labor, that there is a great tendency in the patient to take on those dangerous forms of inflammation, whether there has been undue manipulations used in the delivery or not. Why is it?

Respectfully,

J. P. CHESNEY.

Transactions of Societies.

PROCEEDINGS OF THE ST. LOUIS MEDICAL SOCIETY.

REPORTED BY JAMES W. CLEMENS, M. D., RECORDING SECRETARY.

[Continued.]

CHLOROFORM IN PARTURITION.

Dr. Clemens, in continuing the discussion, read the following paper. Dr. C. begs leave to inform the readers of the Journal that the paper was written under a misunderstanding of the term "destructive change," as used by Dr. W. The reader will percieve that the views of Dr. W. and Dr. C. agree in the main, and the paper is only introduced so that the concluding interesting, and novel views of Dr. Watters may be more clearly understood.

In Dr. Watters' remarks, he based his views of the application of alcohol to the treatment of disease, on its physiological action. This is unquestionably the true source of knowledge relating to the action of all remedies. He does not, however, state by what means he has arrived at the explanation of the action of alcohol according to his definition, beyond the statement of the general law, that as vital power is the result of the destruction of tissue, that which increases destruction will increase power. To quote from the report, "The source of every action in the body being this destructive change, stimulants produce a manifestation of more power, because they occasion the actions which are antecedent. A stimulant is a stimulant only, in that it is the occasion of *increased waste of tissue*. The theory is logically consistent, for as destruction is the source of vital power, that which will increase this power will also increase the destructive process or waste." This all seems reasonable enough, the sequence follows naturally from the premises, and I do not doubt the correctness of the argument, or, indeed, the general truth of the theory. But in thinking on the subject, some facts come to mind which seem to militate against this argument, and it is for the purpose of having these facts explained, that I present them. It is not through any desire of argumentation, or spirit of criticism that I do so, but only in the spirit of an earnest seeker after truth.

The waste of tissue—the result of action—is usually looked upon as effete matter, of no more use to the organism; and although the direct waste, as from the action of a muscle, does not immediately become effete matter, yet after undergoing a variety of changes, and filling certain important duties in its course of metamorphosis, in the end it does become truly effete, and as such is thrown out of the system. The amount of effete matter then, eliminated from the system, becomes the measure of tissue change, and if we can measure or weigh the amount of this elimination, we can easily determine the amount of tissue waste in a certain organism under certain circumstances. By placing this organism under different circumstances, submitting it to different influences, we can determine the rapidity and

amount of elimination under each influence, and compare one with the other. Excrementitious substances, passed as urine, constitute one, and the greatest source of estimating the amount of tissue waste. They represent those portions of the body which really have become effete through action, and their weight will indicate, to a very great extent, the amount of tissue metamorphosis taking place under the circumstances influencing the system. Now let us examine this point in reference to the definition of stimulants.

If an individual, who resides in an inland town, visits the seashore, and spends some time there, he will soon experience the well known effect consequent upon the influence of sea air. His appetite increases, he feels invigorated and strengthened, and increases in weight. By an examination of the urine, in cases where the experiment has been made, a great increase of urea and sulphuric acid is found. These two urinary ingredients represent the disintegration of the nitrogenous and sulphur holding tissues, and of course indicate, in the case cited, increased metamorphosis of those tissues. With the same food as to quantity and quality, the same amount of exercise and sleep, with the same circumstances, as nearly as possible, which surrounded the experimentor in his former residence inland, he will *decrease* in general weight, owing to the fact that destruction is more rapid at the sea side than inland, and the same amount of food will not suffice to keep his body at a given weight; but with more food, all other circumstances being the same, there will be an increase in his general weight. The destructive powers of the system are increased, more food can be assimilated, and a more rapid building up of tissue follows the more energetic unbuilding. The aggressive effect of oxygen on the tissues is heightened, either by the larger supply of air, which the constant and free movement of sea air insures, or by a special quality of the air at the sea side, as by the presence of ozone. The effect of sea air on the system, then, is, in the first place, the production of increased disintegration of tissue—increased waste.

If an individual lives on a certain regulated diet, one which, from previous experiments, suffices to keep his system in a condition

of health, neither gaining nor losing in general body weight, the addition only of simple water in large quantities, will cause a *loss* of strength and flesh to a great degree. If the amount of his food be increased, this loss will not occur, but he even may *increase* both in strength and flesh. In both cases, the amount of the urinary ingredients is much increased; in the one, however, the increased waste is not compensated by an increased supply of material for tissue building, and hence a diminution of general weight occurs; in the other, the waste is compensated, or more than compensated by the additional amount of material added with the increased food. Water, then, acts by increasing the tissue changes; for the diet being unchanged from that sufficient under ordinary circumstances to keep the body at a certain weight, the rapid loss of weight, the increased amount of the urinary ingredients, must come from an increased disintegration of tissue and increased elimination of tissue waste.

If a stimulant is an agent "which is the occasion of increased waste of tissue," and if its power in producing stimulation is proportionate to its power of increasing waste, then sea air and simple water are stimulants of a very powerful character. In both, great tissue waste is the consequence of their use. Sea air has been usually thought a tonic, and thus a stimulant of a low grade, but I am not aware that it was ever claimed that simple water was, or could act as, a stimulant; yet, if the definition be true, it must come under this head.

Now, let us examine into the effects of alcohol as shown by the same method of experimentation. When added to a well regulated diet, what does it do? It lessens the secretion of water by the kidneys, the elimination of urea, uric acid, sulphuric acid, phosphoric acid and chlorine. There is not simply a retention of these substances for a time within the system, (except chlorine,) to be afterward passed away, but an absolute diminution in their formation, and consequently a diminution in the tissue metamorphoses by which these substances are formed—a diminution of waste. How alcohol acts in this retardation we cannot say, but, if the urine can be taken as the measure of tissue

metamorphosis, as the measure of the amount of those effete matters resulting from action of the tissues, then alcohol cannot come under the definition of a stimulant as given by Dr. Watters.

The prize fighter, in his preparation for the ring, has, by long practice, acquired empirically the very principles which would most aid him in gaining a perfect physique. He eats largely of animal food, drinks plentifully of pure water, sleeps regularly, and takes a gradually increasing amount of exercise; and thus he brings into play all the most powerful causes of tissue change. Metamorphosis is rapid, and tissue-building is also rapid, and thus he polishes and perfects to the greatest degree the whole animal economy. From our present physiological knowledge of tissue change, the reasonable deduction may be made, that, the more rapid the metamorphosis of the tissues, within certain limits, the more perfect is nutrition, as long as nutriment is supplied in sufficient amount, and as long as the formative power can use it. How does the prize-fighter act in regard to alcohol? He violates the first rule of his training by using it; he discards it in all its varieties except, probably, a limited quantity of weak beer which has a different action from alcohol. Did alcohol possess the power of increasing tissue change—supposing the law which has just been announced to be true—then its use would be advantageous to him; but from its action in retarding and lessening tissue change, it would prevent that active destructive process and more active building-up power, by which the organism is perfected, and hence he avoids it. Now, we have a certain definition given of a stimulant, or rather of its mode of action; we have cited one substance, at least, which has never been regarded or used as a stimulant, yet its mode of action exactly fills the various parts of the definition. We have also cited another, which every one regards and speaks of as a stimulant, yet its mode of action is opposite to that given in the definition. How will this contradiction be explained? Is the definition incorrect, or is alcohol not a stimulant?

I do not pretend to explain the contradiction, I only present the points for solution by those more able than myself. I may

be permitted, however, to say that in the very nature of things, we cannot expect to know much of the ultimate action of any special medicine, simply from the fact, that we know so little of the chemico-physiological changes continually taking place in the body. Physiological chemistry is yet in its infancy, but making rapid progress towards perfection. The facts it has presented are too few to enable us to generalize as yet, but a few years, it is to be hoped, will bring us sufficient to theorize upon a true basis.

I have never had the pleasure of hearing the opinions and theories of Dr. Watters in regard to life, what it is, and how constituted. But I should judge, from the character of his remarks made at last meeting, that he is, at least, strongly inclined to adopt that doctrine which explains life by the combination of those laws governing material bodies, and which science and inquiry are gradually unfolding. Judging from his remarks, also, I should think that he is inclined to discard what is generally understood as vitality, as an element of life, but views it rather as the result of the combination of those laws, examples of which he gave. I cannot make up my mind to discard such a force as I deem vitality to be, as an element of life. It is the most important of all the forces operating in the production of life. The name, however, is unfortunate, for it includes within itself, as generally understood, all the phenomena of life, and hence, in reality is life itself, and not a part. The term vitality I am disposed to change for another, which, though not more clear or intelligible in itself, yet, as a term not in such general use, will not be so liable to mislead. I allude to the term "germ or cell force"—that peculiar power which is implanted in matter by fecundation. If I was called on to define what I understood by life, I would answer that, "life is the result of the action of various forces, correlative to the forces which operate on inorganic matter, controlled and directed by a peculiar organic force, originating from a parent, and termed cell or germ force." This, I am aware, is not a clear and satisfactory definition; but can one be given which shall be without an objection? I only offer the definition as giving my ideas of life. That life is complex, I

have no doubt; that the ordinary forces which operate on inorganic matter are intimately connected with the production of the phenomena of life, I have less doubt of; but I am thoroughly convinced that with all the other forces in their most perfect condition, they would only produce a mere mass of inorganic matter, were they not so directed by germ force, as to combine all their various actions into one harmonious whole. Therefore, with this view of life, the actions constituting it must be multiple and various, and the explanation of the ultimate action of any single remedy exceedingly difficult. Life not being made up of one force alone, but several, medicines which, from the general result of their action are combined into one class, may reach the end by different routes,—they may differ in the force upon which and through which they operate; and thus an explanation which is thought sufficient to cover the whole class, may be erroneous in respect to some. That alcohol is what is generally termed a stimulant, I cannot doubt; but that it acts by increasing tissue metamorphosis, I cannot believe. May not alcohol act, and act as a stimulant, not directly by increasing tissue change, but indirectly, by lessening this change and economising that expenditure of germ force engaged in the change? Germ force —that power which is inherent to a cell, and which we contend is so necessary to cell growth and change—like all other forces, is limited in the amount of power which it possesses; it can be exhausted by rapidity of action. With its decrease and cessation, the various actions constituting life cease to produce the usual phenomena of life, and the organism, uncontrolled, passes under the influence of laws governing inorganic matter. Increased tissue change, as we have shown, when accompanied by an increased supply of nutriment, if the formative power is not lessened, will be followed by an increase of strength. But in disease we do not find these conditions to exist; the functions of the various organs are imperfectly performed, less nutriment is assimilated, and increase of waste will be followed by exhaustion and death. If we can decrease the amount of waste, if we can husband the expenditure of cell force, we lengthen the life of

the patient, and enable him to pass through the changes of disease more securely. Thus alcohol, by decreasing the amount of tissue change, lessens the expenditure of the force engaged in its production, and thus sustains the organic power of the system; and although not acting by increased metamorphosis of tissue, would still act as a stimulant.

Dr. Watters, in reply, read the following:

In considering the very interesting paper of Dr. Clemens, read at the last meeting of the Society, in relation to some views I expressed at a former meeting, as to "the modus operandi of stimulants, their indication and contra-indication in disease," I would remark, that it seems to me that if I could be so fortunate as to express myself clearly, so as to be understood, it would be manifest that the facts presented in the paper referred to, are not only not opposed to my position, but really confirmatory of them. I hold that *all* vital phenomena or changes in man, are reciprocal with oxydation, or with the recoiling of organic compounds back towards inorganic compounds. As a deduction from this general proposition, I hold that all stimulation or increase of action, must be, and is, the result of increased destructive changes, and a stimulant is a stimulant, in that it is the occasion of this increased recoiling. That, hence, stimulants are naturally divided into two great classes,—first, those that occasion the increased oxydation of the existing organic constituents of the body, and secondly, those that furnish additional material to the body for more rapid recoiling. That this distinction was of the most vital practical importance, because these two classes could not be used indiscriminately in depression without injurious, and often fatal results. Depression, or diminished vital action, is the result of diminished oxydation, and may be due,—first, to diminished material for oxydation, or, secondly, to abnormal conditions for the oxydation of the material already in the body. I hold that the depression incident upon the use of anæsthetics, is of this second class, in that the oxygen of the blood is seized and consumed by these agents; the depression *here* being not for want of material to recoil, but for want of oxygen. Stimulants of the second class are contra-indicated, for they would

increase the sedation and the danger. Here we come to a most important practical point,—that a "stimulant," so called, may be a sedative, and that mere depression of a patient is not sufficient reason for the use of agents thus classed in the books, without great discrimination and judgment. If this view of life, of stimulation and of sedation be correct, we may reasonably hope that the study of these agents, as to the modus operandi of their individual effects, will be followed by good practical results.

Dr. Clemens, as indeed most others, represents "waste as the *result* of action." I maintain that waste, or the recoiling of organic compounds towards the inorganic by the action of oxygen, is the antecedent motor to the vital motions in man. His case cited, of the influence of sea air upon an invalid from an inland town, corroborates this view, while *I* cannot see how these facts can be reconciled with the doctrine that waste is the result of vital action. If the vital force is expended in action, and the increased waste is due to the fact that the tissues are thus more surrendered to the tender mercies of the chemical forces, how, I ask, can the increase of the conditions of waste become a tonic, as in the case cited? A man leading a sedentary life is feeble and badly nourished; he is empirically recommended to take exercise in the open air, and he improves; his strength and nutrition are improved by that which increases the demand. This is a paradox, but its truth rests in the fact that waste is the motor of life.

So the facts cited by Dr. Clemens as to the effects of alcohol upon the solid constituents of the urine, are confirmatory of my positions and would be inferred from them. Alcohol being a stimulant of the second class, in that it is a material for more rapid oxydation, and the supply of oxygen being limited, there would be a diminution of the waste of tissue, in proportion as the oxygen united rather with the carbon and hydrogen of the alcohol. Though the solid constituents of the urine are diminished under the influence of alcohol, the carbonic acid thrown off by the lungs is greatly increased. Thus, so far from the facts adduced by Dr. Clemens militating against my views, I am unable to see how they are consistent with any other theory. But I may explain myself better by pointing out the general principles indicating the use o-

stimulants, as deduced from the more general theory of life, and, as I think, confirmed by my experience and the experience, of others associated with me during the last few years. In my first remarks I only attempted to point out the contra-indication for alcoholic stimulants in certain states of depression, as that from anaesthetics; that in depression of the second class, depending as it does upon the absence of conditions for the proper oxydation of the constituents of the body already existing, the administration of stimulants of the second class, which only add to the difficulty, is contra-indicated.

But most diseases are to be removed, if at all, by the very processes of life themselves. Now these processes involve an expenditure of power which is necessarily represented in antecedent destructive charge. In health, this continual destruction is provided for by nutrition; the springs as they recoil and thus move the machinery of life, are loosened and removed from the body by the emunctories, while other springs, *wound up* in the vegetable world in the form of albumen and fibrin, fat, sugar and starch are introduced, digested and absorbed, and *screwed on* in nutrition, themselves in recoiling to continue the movements of life. But in disease, this source of continual supply is cut off to a greater or lesser extent by the derangement of the digestive and nutritive processes, and our patient is thrown back almost entirely upon the storehouse of his own body for that very power by which the disease is to be removed. His great hope is, that this may hold out until this object is effected, and a supply from without in the form of food can be appropriated. We cannot be too careful, therefore, how we rob our patient by depleting agents of this his last and only hope. And we cannot be too careful to protect him from every useless expenditure of power; he should be allowed to use no voluntary muscular exertion; he should be protected from all mental excitement, and everything that implies a loss of structure and expenditure of power, not appropriated to the removal of the disease. Exercise is a stimulant of the first class, and a tonic when the increased power is appropriated to the purposes of life; but in acute disease it cannot be so appropriated, and is consequently a prodigal and suicidal waste.

Stimulants of the first class, that is, those which occasion an increased waste of the sources of power already in the body, are indicated only when this waste is *below* that point where the greatest and best results are effected with the least expenditure. To illustrate: suppose an engine supplied with a definite amount of coal, and it is required to do the greatest possible amount of work with this limited fuel, there is a fixed and definite rapidity, subject to mathematical calculation, with which the coal should be consumed or burned, to effect this object. It might be all consumed without causing any motion of the engine—without even raising the water to the boiling point; then it might be burned so rapidly as to produce but little motion of the engine, except, perhaps, an explosion. So this class of stimulants can be indicated only when the waste is below that point where the greatest results are effected with the least expenditure. Thus, in inflammation and fever, for instance, as the power developed in waste already existing is beyond that point of parsimony, these stimulants are contra-indicated, and their use would be injurious.

But the indications for stimulants of the second class are different, and, in many respects, opposite. The *excitement* of inflammation and fever is not in itself injurious, but rather the too rapid and destructive waste of tissues and organs which this excitement indicates. Thus we are not to fear the temporary increased excitement occasioned by these stimulants, for this increased excitement is at the expense of the carbon and hydrogen administered, to a corresponding relief of the tissues and organs diseased, from the destructive changes in which danger is to be feared. Hence alcohol is an antiphlogistic, in that it acts vicariously for the diseased organs, and at the same time, in its own recoiling, develops that power which is represented in the *vis medicatrix naturæ*. Thus the powers of life are conserved, and the disease indirectly lessened, the danger of which consists in a too rapid destruction of tissue. So alcohol in large quantities is an antidote for the poison of snake bite, and as ether acts more rapidly, I would expect it to be more efficacious.

The Doctor concludes his paper with some remarks upon "Life, or Germ Force," and I confess that on this subject the whole

discussion turns. My views of the indications and contra-indications for stimulants in disease, are deducible from what I consider germ force. He remarks, "If I was called on to define what I understand by life, I would answer that life is the result of the actions of various forces correlative to the forces which operate on inorganic matter, controlled and directed by a peculiar organic force, originating from a parent, and termed cell or germ force." But what *is* this cell or germ force? First, it *controls and directs*. Upon that we agree. But to control and direct, it must either be self determinate, or it must not be self-determinate, but determined by somewhat not itself. To be self determinate, it must be conscious intelligence, because it determines itself to so control and direct as to form the eye, the lungs, the heart, &c. Every cell of the most complex organism must have this conscious intelligence, for every cell lives for and by itself in the same sense as does the original germ, or the full grown man. But this is absurd, because the vital actions accord with fixed laws. Vertebrate animals are developed upon the same plan over the earth, and in all time, so far as the remains are found; the eyes and lungs and heart, in accordance with these laws, are formed as perfectly in a fœtus without a brain; or, if fecundation occur in the fallopian tube, development will go on though it involve the death both of the fœtus and mother. Then, as this force which controls and directs is not self determinate, it must itself be controlled and directed by not itself; then it is "*not itself*," or not this force that controls and directs. Now this continual *not itself* in space and time, is matter or form, and motion. Here we have the identical doctrine of cell or germ force, with which I began my remarks at a previous meeting. This continual "*not itself*," expressed in other words, is the universal law which I announced, and which was announced long before me, even by the ancient Greeks, that all existence is in negation. Applied to the phenomena of life, the germ, *with* its surroundings, controls and directs —the *mode* is determined by the form, and the motion by antecedent motion.

Dr. Carpenter involves himself in the same contradiction when he applies the correlation theory to an explanation of life. With

him, the physical forces, as heat, light, etc., are converted by the germ or organism, as a "*material substratum*," into a peculiar force which he feels himself compelled to assume. Here, too, it is the germ or organism that controls and directs, according to his own thoory, and not his peculiar force; for this is itself necessitated and determined by the germ or organism.

I have so far only attempted to determine, in a *general* way, the indications and contra-indications for the use of stimulants and sedatives, and have not attempted to apply the principles to individual cases, except for illustration. But for fear of being misunderstood, as the present is a transition period in medical doctrines, I would remark, that after making a correct diagnosis, it is of the first importance, as a guide to the proper treatment, to determine the *direction* in which dangerous or fatal results are to be apprehended in the particular case. Suppose the case pneumonia, for instance. All inflammation is too great destructive change, in proportion to nutrition. This may exist in every different state of system; but in different states of system the fatal results are to be apprehended in totally different directions. The indications of treatment are to diminish destruction and to improve nutrition; but to which of these indications we are especially to direct our energies, at the time, depends upon the stage of the disease and the direction of danger. If the patient had been previously well and stout, and the disease promised to be of a sthenic charactsr, and had not passed the first stage, I would apprehend danger, not in the exhaustion from the consumption of the material of life stored up in his body, but rather from the organ becoming overwhelmed, and from the destruction of a portion essential to life. Here I would not hesitate to bleed freely and purge; and at the same time, for reasons above stated, give whiskey or brandy in sedative quantities, if the skin is hot and florid and pulse frequent and excited. But if the skin is purplish and cold, and a state of depression exists evidently from imperfect respiration, alcoholic stimulants would be contra-indicated as in depression from anæthesis. This plan of treatment is mainly directed to the first indication—to diminish the destructive change—and is inidicated under these circumstances,

though it diminish the strength and vital resources. But, on the other hand, if the blood is impoverished or poisoned, and the disease asthenic, nothing should be done to lessen the vital resources; but the remedies should be mainly directed from the beginning to the second indication,—to improve nutrition and to sustain the vital energies, while we diminish destructive change only by such agents as do not tax the remaining resources.

CURVATURES OF THE SPINE.

Dr. HAMMER remarked that orthopoedic surgery was a branch of surgery in which there was much room for improvement, and every observation made in practice should be taken hold of to improve ourselves in that branch. In regard to curvatures of the spine, an opinion has long been prevalent, that the deformity may be brought about and maintained by bad habits, as by carelessness in attitude. Researches by surgeons within the last two years have made the conclusion probable, that this general belief is not true, as a rule. We know very well, that permanent curvature of the spine depends upon a change in the structure of the vertebra and cartilage. The usual cause of this change is ostitis, resulting in softening, when the superincumbent weight compresses the softened portion, and alters the shape of the vertebral column. Sometimes the inflammation results in abscess or suppuration, in which event the cases usually are fatal, and are not enumerated among the cases of permanent curvature. After the bone has been thus softened, it is again hardened by ossification, but the original form of the bone is not restored; it simply becomes firm in its altered shape. In this way we have true permanent curvature. In order to show that this doctrine is correct, and that bad habits will not cause a permanent curvature, he related a case upon which he operated a short time ago, understanding at the same time that it was not an isolated case, but one among many of a similar character, occurring in his practice during the last fifteen or twenty years. The case was that of a girl, who was presented to him affected with "caput obst cum" on the left side. The contraction of the sterno-cleido muscle

was so great, that the chin was almost in contact with the chest, and fixed in that situation. The first rule to be observed is to ascertain if there is any pliability of the cervical vertebra, as upon this fact depends the probability of a successful operation. If the vertebra is immobile, no operation can be performed, but if otherwise, a successful one can be anticipated. In this instance the fact could not be determined, as the muscle was so firm in its contraction that it could not be moved. He operated, however, without hesitation, from past experience in such cases. Immediately after cutting the muscle he could at once move the head, and found the flexibility of the spinal column perfect. This is an instance proving one fact, viz: a certain part of the spinal column may be held immovably in one position for a number of years, and yet the flexibility of the column may be at once restored by cutting the cord which holds it in that position. Seventeen years ago he saw a patient who had suffered from boyhood from the same deformity, and the same happy result had followed an operation. His cases varied, in the length of time during which they had been affected, from six to twelve years; one was sixteen, and one twenty years. In all, after division of the muscle at fault, the flexibility of the spinal column was found to be unaffected. If after an operation by which the head is straightened, it falls back to its old position, it is not from any fault of the spinal column, but from habit, and this can be overcome in a short time. This experience is not only important, but consoling, for we see that as there is no disease of the vertebra or cartilage, we can promise a radical cure. What is true with respect to the sterno-cleido muscle, will hold good in other muscles. The most important point to determine, is the muscle or group of muscles which are at fault; as soon as we determine this, and are satisfied that the vertebra are free from ostitis, so soon will we be able to restore the spinal column to its normal position. He also alluded to two other facts in connection with caput obsticum: 1st, that all the cases coming under his observation dated back to infancy, either congenetal or were acquired during the first year after birth. 2d, that the side of the face,

even to the eyelids, toward which the contraction took place, was smaller than the opposite. When the patient is grown, this disproportion remains permanent. Another fact, that in all the cases except one or two, the disease existed on the left side. All his cases corroborated the views here advanced, viz: that in all cases in which ostitis existed, the deformity will be permanent, whilst curvature from contraction of muscles alone can be removed.

EMBRYOTOMY.

Dr. PALLEN, in response to a desire expressed to hear his views upon the interesting and important subject of embryotomy, remarked, that the operation of embryotomy should not be *performed* when there was a rational prospect of saving the life of the child, without exposing the mother to extraordinary hazard. The operation of embryotomy should not be *attempted* when the completion of labor would be impracticable, per vias naturales. There are various conditions requiring instrumental interference, 1st, where there exists deformity of the pelvis. He made three divisions of deformities of the pelvis; first, where the congugate diameter of the superior strait is less than four inches, but not over three inches; second, when the diameter is less than three and not above two; third, where it is less than two. In the first class of cases, labor coming on at full time, there is danger both to mother and child. The danger to the child may arise from two causes; first, from compression of the brain. This danger has been overrated, as the brain proper is not of much consequence to the child; as children have been born without a brain. The principal danger is from compression of the placenta and cord producing asphyxia in the child. If labor be long continued, this source of danger to the child will be very great. There are dangers also to the mother. The soft tissues of the mother, pressed between the head of the child and bones of the pelvis, bear the contusions badly; and inflammation and sloughing may arise, so that it is not well for us to trust long to nature. It is true that cases have been reported where children have been born, although the maternal pelvis was deformed; but these are excep-

tions to the rule. The children were probably born before full time, or had small heads. In the cases reported, no measurement of the head was given; nor was it stated whether the woman was at full time. It is also true, that children have been born dead through the efforts of nature alone; but they had been, doubtless, dead for some time, and the bones of the head being flexible, they would pass more readily. These cases furnish no rule for guidance. What are we to do when called to such cases? Trust to the efforts of nature? None but timid practitioners would rely on this. Of what means may we avail ourselves to aid delivery? We may turn and deliver according to the views of Dr. Simpson; we may attempt to deliver by the forceps; we may perform the Cæsarian section; or we may operate by embryotomy. As it would be unwise to trust to the efforts of nature, shall we use podalic version? It has been argued by Dr. Simpson, that a cone with its apex presenting, can be dragged through an opening more easily than if the base offered. Even under the most favorable circumstances surrounding footling cases, thirty-three per cent. of children perish. In cases of deformity, the mortality would be at least as great, besides the following objection: According to the representations of Dr. Simpson, the apex of the cone—the Bi-mastoid diameter—is three inches, the diameter of the arch, or cranial vault, is three and a half inches. If the base of the skull—the apex of a truncated cone—be three inches, how can it pass through a pelvis, the diameter of which is three inches? Besides, the measurements were made on the naked skull; if the soft parts be added, a still greater disparity would be shown. He thought the operation was one to which very few would resort. The next aid mentioned is the long forceps. Very excellent authority advises their use, among others Dr. Hodge. Is this means practicable, however, when the antero-posterior diameter is three inches and the diameter, of the child's head engaged in passing, three and a half to four inches? Even if they be applied as has been recommended, over the frontal and occipital bone, the bi-parietal diameter will be increased. Dr. Bedford, who is favorable to the use of forceps generally, is

opposed to their use is such cases. English authorities are opposed to their use, except a few individuals. The Irish also oppose them. In fourteen thousand cases of delivery in Dublin, the long forceps were never used once. It is true that Dr. Hodge and others favor this method of delivery, and it is also advocated by the German and Spanish authors; but, on the other hand, in Paris, the most eminent authorities oppose it. Statistics prove that when the long forceps are used, they are productive of harm, and Dr. Bedford states that more children have been destroyed by the long forceps, than saved by them. He was opposed to their use. If, then, we cannot trust to nature, to podalic version, nor to the long forceps, what shall we do? Shall we attempt to save both mother and child by employing the Cæsarian section? or shall we view the mother's life as paramount, and perform embryotomy? This question is an exceedingly delicate one, and every one must decide it for himself. Dr. Churchill's statistics of embryotomy operations in Great Britain, give 1 in every 291 cases of labor; France and Italy, 1 in 563 cases; Germany, 1 in 1,675. Taking all together, 1 in 1,120 cases. The mortality among the mothers was stated to be 1 in every 5, or 20 in every 100. What is the mortality in the Cæsarian section? Dr. Denman stated that nearly all women perished. Ramsbotham says that 45 out of 50 died, or 90 per cent. West reports 83.6 died in 100. Churchill has collected the records of 79 cases in England and America; of these 56 died. In France, Baudeloque reports 24 successful cases between 1750 and 1800; the unsuccessful cases not reported. Valpeau places the mortality at 50 per cent. Madame Lachapelle stated that every case at the Maternité terminated fatally, but collected the records of other cases, 16 in number, of which only 4 died. Spangle, in his history of the Cæsarian section, reports 106 cases,—61 recoveries and 45 deaths; mortality, 42 per cent. Baudeloque gives 42 out of 73 cases. Moore thinks 1 woman in 14 is saved. Cazeau stated that four-fifths of all cases die. Keiser states that 79 per cent. terminate unfavorably. Cazeau asserts that the operation is only justifiable in extreme cases, when embryotomy cannot be

performed. Is it reasonable, then, to perform this operation in preference to embryotomy? His own opinion was against it: he preferred embryotomy.

In the other division of the deformities of the pelvis, when the antero-posterior diameter is less than three but over two, the hope of aid from nature, turning or the forceps, is less than in the first division. The question is only between the Cæsarian section and embryotomy. In such cases he would prefer embryotomy. In the third division, even if the diameter was fully two inches, he would not prefer embryotomy, provided the child was at full term. It had been stated that children had been delivered by embryotomy when the diameter was one and a half or one and three-fourth inches, but we know that when the head is broken up, we cannot deliver the base through an opening less than two inches; hence, in cases of the third division, whether the child be dead or alive, delivery *must* be accomplished by the Cæsarian section, and the sooner it is performed the better. As soon, therefore, as the os uteri is open, and before the membranes are ruptured, the operation should be performed.

There are cases which sometimes occur, where there is no deformity of the pelvis, but the labor is tedious and bad symptoms supervene; such as indications of inflammation or exhaustion. Whilst the head is above the superior strait no bad symptoms can arise, and, therefore, there can be no necessity for instrumental interference, provided the pelvis is large enough to permit the head to pass. Bad symptoms can only arise after the head has been engaged in the superior strait. Shall we use the long forceps under such circumstances? Most practitioners will apply them, but English authorities, and some French, oppose their use, and contend that the ears of the child should be felt before any application should be made. Dr. Pallen advocated the same opinion. There were more failures than success in the use of the long forceps at the superior strait—more children destroyed by them than saved. If the ear can be distinguished, and bad symptoms come on, he would apply the forceps and deliver. If the head was arrested, when bearing on the perineum, of course, he would deliver with forceps.

Dr. NEWMAN had no objection to offer to the statements of Dr. Pallen, but desired to mention the importance of auscultation before obstetrical operations.

It was stated by Dr. Bland, a short time ago, that he had seen a case in which craniotomy was resorted to. He was present, also, and was satisfied that delivery otherwise was impossible; he was also satisfied, by auscultation, that the child was dead, and hence advised no delay. It was a question in morals, if it was proper, under any circumstances, to sacrifice the child to save the mother. Some asserted it to be improper under all circumstances, but a large majority of the profession have adopted the opposite view, and do not hesitate to sacrifice the child, when the life of the mother is in jeopardy. He was averse to destroy the life of a fœtus, but believed it to be the duty of the accoucheur sometimes to do so. We can almost always decide, by means of auscultation, whether the child is living or dead; and a knowledge of the child's death will take away the repugnance every one feels towards the operation of embryotomy. He had made use of it in several cases with much satisfaction.

Dr. PALLEN had not referred to auscultation, in his remarks, as he had not deemed it necessary. Every one present, in the cases referred to, when the head was high up, would satisfy himself if the child was living or dead. When the head was low down the forceps can be so easily applied that he did not attach much importance to auscultation. If the head should lie high up, and the child dead, a careful man would prefer the forceps. The signs from auscultation, however, are fallacious, more so than is usually supposed. Other signs, also indicative of the death of the child, are not to be relied on positively—the passage of meconium, for instance. He had seen cases where the head presented and the meconium was passed, yet the child afterwards born alive. Emphesyma of the scalp — the caput succedaneum—crackling under pressure, the bones of the head also overriding, had been spoken of, but this also was fallacious. The case may be one of hydrocephalus, and the child born alive. It is a fact, that sometimes we do not hear the sounds of the

fœtal heart, though heard previously, and yet the child be born alive. After the rupture of the membranes, and the uterus has contracted around the child, it had been stated that the cessation of the heart's action indicated death; but we now know that, in cases where the best auscultators were unable to hear the sounds of the heart, the child was afterwards born alive. After hearing the heart sounds just before the commencement of labor, and being unable to distinguish them after labor had fully begun, he would be very apt to conclude that the child was dead, especially if other signs were also present. If called to a case in which he had never heard the beating of the heart, he would not conclude, from the absence of heart sounds, that the child was dead. He made it a rule, generally, to listen to the sound of the child's heart, particularly of late, after seeing it stated that children may be born alive, after the attendant was unable to distinguish the heart's action.

ULCERATIONS OF THE OS UTERI.

Dr. JOHNSON brought before the attention of the Society the article by Dr. Marion Sims, in the London Lancet for June, 1865, in relation to diseases of the cervix uteri, and gave a resumé of the treatment by incising the cervix, principally for congestion dysmenorrhœa, ulceration and sterility. The article relates, in the warmest terms, the advantages and success of this method of treatment.

Dr. BOISLINERE made some remarks on the method of treatment proposed, and dissented from some of the views advanced in the article. In ulcerations, he thought, a distinction should be made between those occurring primarily, and those produced by conditions of the general system, and by chronic metritis. Where congestion is present, in cases of chronic metritis accompanied by ulceration, he preferred antiphlogistics—four or five leeches twice a week. In the beginning, when the blood is rich, he employed saline purgatives, for a time, and then gave bichloride of mercury with iodide of potassium, at the same time using leeches if necessary. He also employed the hip bath, temp. 85°, not above it, as the

congestion would be increased by a greater temperature. Vaginal injections, composed of infusion of poppy heads, the aqueous extract of opium, gr. ij. to 0j. or demulcent and medicated pessaries, were favorite applications with him. When congestion was present, he used narcotic pessaries, x grs. aa of ext. belladonna, and ext. conium, with Zj. of mercurial ointment, made into a ball with cacao butter or spermaceti, sufficiently large to enable it to pass through an ordinary glass speculum. By persevering with these means, success will generally follow. He varied the pessaries, sometimes, using in their composition iodide of potash, or lead; the latter, he thought, excellent in discussing glandular tumors in any situation, sometimes even in three or four days. He also employed counter-irritation, according to Edward Rigby —tartar emetic ointment to the groin or sacrum. In cases of passive congestion of the organ, with a low state of the system, he used tonics and a sustaining and astringent course of treatment—applying perchloride of iron locally, also administering it internally. He thought this preparation of iron gave tone to the capillaries, and thus acted indirectly as an antiphlogistic. The operation referred to by Dr. Johnson, he thought, only applied to cases of dysmenorrhœa and sterility. He recommended very highly, in cases of dysmenorrhœa, a mixture of equal parts of turpentine, black drop and spirits of camphor, given in thirty drop doses.

Dr. GREGORY was more disposed to favor dilatation in preference to incision. Had used dilatation in strictured urethra for years past, and found it to be entirely successful. He was opposed to the knife, except when absolutely necessary. In strictures of the cervix, a sponge tent, introduced in the evening, would dilate the canal sufficiently by next morning. When much congestion was present, incisions would relieve that, but would not cure a constitutional ulcer.

Review of Current Medical Literature.

PRACTICE OF MEDICINE.—(CONTINUED.)

7—*On the Treatment of Rheumatism.*

"A very interesting point," says the editor of the Lancet, in a recent article, "in practical medicine, which some of us may have fancied as well nigh settled, would seem to be still a matter for difference of opinion. We refer to the treatment of acute rheumatism. Few diseases are more important than this. When ill-treated, few have more power of doing irreparable harm to the patient; when well treated, few have more power to raise the medical profession in the opinion of mankind."

Among the various lines of treatment which recent laborers in this field of therapeutics see reason to praise, some are the old, well-beaten paths, strewn with new observations and repaved in places by new modifications; others are entirely new. Blood letting and colchicum are again recommended by Dr. Rob. Law, (*Dublin Journal*, XXXVII), and declared to be the most rapid and the safest method:

"It consists in a moderate venesection (but seldom exceeding 8 oz., and very rarely repeated,) and in the exhibition of colchicum in the form of the tincture or wine (usually 2 drachms in an 8 oz. mixture), or the acetic extract (3–4 times daily 1 grain). Cathartics are given only in case of necessity (mostly senna in comb. with colchicum); in case of severe pain, Law combines with it opium in large doses. Upon the affected joints he applies tinct. iod. and blisters. In endo- or pericarditis he uses, according to the stage, digitalis or calomel. * * * When the acute phenomena and the fever have passed away, he gives cinchona, or quinine and iodide of potassium, and, in case of remaining stiffness in the joints, warm baths—while the latter only act injuriously in the inflammatory stage."—[*Schmidt's Jahrb.* CXXVII p. 86.]

The alkaline method has been strengthened by a modification and renewed praise. Dr. B. G. Everett, (London Lancet), recommends it in the form of a bath. He says:

"Having for the past seven years been, I might say, a martyr to rheumatic fever, very few months passing by (till within the last year) without my being

confined to bed, perfectly helpless, for some weeks. I need not say I take a peculiar interest in the treatment of that disease; and having, I quite think, warded off two attacks within the past twelve months, I shall feel obliged if you will insert this in your journal, in the hope that some of your readers will be induced to adopt the treatment in similar cases, and kindly inform me, through your pages, the result.

"During a severe attack of rheumatic fever about fifteen months ago, I found nothing relieved the pain or apparently did so much good amongst local applications as spongio-piline soaked in a warm saturated solution of nitrate and carbonate of potash, and applied to the joints; so I determined that when well again, directly I had any rheumatic pains, I would have a warm bath containing potash. As usual, only a few months passed; all the early symptoms of my old enemy returned. I was unable to use my extremities, and also had lumbago, showing there was both muscular and articular rheumatism. I at once decided to try the bath; it contained about twenty-four gallons of water, at 100°, two pounds of bi-carbonate of potash, and one pound of nitrate of potash. I remained in it ten minutes. The following morning I was much better; there was no fever whatever, and in a few days I was well, and able to attend to my practice.

"Shortly after this period I had a patient with rheumatic fever, and it being in its early stage when I first saw him, before the joints were much swollen, I ordered him a similar bath to the one I had, and with a similar result. I also gave it to a little boy, who when I saw him was delirious, although the joints were not much swollen, and there was no muscular rheumatism or cardiac mischief. In less than a week the boy was down stairs.

"About a fortnight ago I again had all the premonitory symptoms of acute rheumatism, but at once had the alkaline bath as before. In two or three days I was well (although the knees were stiff and painful for a week), and had not a symptom of fever.

"I doubt not I have to thank the bath for saving me from two attacks of rheumatic fever; for until I used the alkaline bath whenever I had any rheumatism about me, it was sure to run on to the fever, and keep me a prisoner for several weeks."—[*Lancet, Jan.*, 1865, *p.* 82.]

Dr. Chabassu (*L'Union med.*,) considers rheumatism *not* as an inflammatory, hypersthenic disease, but sees in the frequent absence of fever, the striking paleness of the mucous membranes, the occasional occurrence of blowing murmurs in the veins, and of nervous disturbances, indications of a decided anæmia. The treatment by large doses of *quinine* has been very justly recommended, but not because of a depressing action upon the nervous function; but, on the contrary, because in these large doses it strengthens the nervous system. The author always introduced this treatment by the use of calomel or of alkalies, to counteract

the fibrinous condition and thickness of the blood, which is likely to cause stasis in the capillaries and local inflammations. He regards the simultaneous use of quinine and digitalis as irrational, because the two remedies have contrary effects. (*Schmidt's Jahrb.*, cxxvii, *p.* 26.)

A combination of *quinine and iodide of potassium* is strongly recommended by Dr. Lemire in the treatment of *inflammatory rheumatism*. In an article published in the Montreal *Gazette Medicale*, he relates the case of a man, 36 years of age, of strong plethoric constitution, suffering from inflammatory rheumatism, affecting the ankles, knees, wrists and shoulder. Twenty grains of jalap were administered with five grains of calomel, followed by the following mixture:

R. Quiniæ sulph..... Zj.
Potassii iodid..... Zij.
Acid sulph., dil..... f.Zss.
Aquæ..... f.Zviij. ft. M.

A tablespoonful every four hours; lemonade as a beverage. After six days of this treatment, the patient had left his bed and could walk about, only feeling a little weak. Two days later, he was perfectly cured, and has had no relapse. In two other cases treated similarly, the disease lasted only eight days. (*Med. and Surg. Reporter*, *vol.* xiii, *p.* 335.)

The most recent and novel therapeutical experiment in rheumatic affections is the "Blister Treatment" of Dr. Herbert Davies (*London Hosp. Rep. I. p.* 293, 1864), which consists in the extensive application of blisters to all affected joints, without the aid of so-called specifics. In all cases he effected a rapid abatement of the symptoms, rapid recovery, and immunity from cardiac affections. He explains these results, pointing to the presence of an acid substance in the blood of rheumatic patients, by assuming that extensive blisters remove this material from the blood, in the serum of the blistered surface. The abnormally acid secretions of the skin, kidneys, etc., soon became neutral or alkaline under the use of blisters; and two recent cases of disease of the heart were cured, while no new exudations upon the valves were thrown out.

Dr. Jeaffreson has successfully treated a number of cases, by this method, in St. Bartholomew's Hospital, and comments as follows:

"The safety of the heart is undoubtedly the main point at which every treatment must be directed; and in this particular, more especially, does the Blister Treatment exhibit its peculiar value. In a communication read, Wednesday, March 22, by Dr. Davies at the Hunterian Society, he stated, that of 50 cases which had been admitted under his care at the London Hospital 27 had hearts already damaged by recent or old inflammatory mischief, and 23 were free from cardiac complication. The results of the blister treatment in these fifty cases showed that as many as twenty-five, when discharged from the Hospital, were totally free from any endo- or pericardiac disease; or, in other words, that while every heart was saved which came in sound, two recent cases of endocarditis were apparently cured by the alteration effected, as he believed, in the alkalinity of the blood by the free discharge of serum from the neighborhood of the inflamed joints. Dr. Davies also states, that those cases answer best to the treatment in which a great number of joints are simultaneously affected, and when, by setting up a large amount of discharging surface in the proximity of the inflamed parts, a large proportion of the *materies morbi* may be evacuated at one *coup*. Cases where the poison would appear to crop up to the surface by instalments, attacking the various joints at intervals of days, do not afford such striking examples of the efficacy of the treatment. The first case, where an unexampled amount of blister was applied in an extremely acute case, and where the patient was discharged, cured in thirteen days, will well illustrate Dr. Davies' position. That this treatment is not simply local in its action is also shown in the alteration produced in the urine in the majority of the cases cited; for in 11, the urine remained acid, but generally diminished in acidity during the whole period of the case; in 22, it became neutral shortly after the serum was discharged; in 10, it exhibited an absolute alkaline reaction; while in 7, no notes were taken.

"*Case* 1.—Wiliam S., aged thirty-two, a working silversmith, and exposed to great variations of temperature, was admitted into the Hospital on December 2, the seventh day of his illness, and was discharged cured on December 15, thirteen days after he came under treatment. Eleven blisters amounting to 482 square inches, were applied simultaneously, and with almost immediate relief. As the patient said, 'the rheumatic pains left me as soon as the blisters drew;' and on the third day from admission all pain had disappeared. The pulse fell from 105 to 95 per minute; the temperature from 101·4 to 99·6 and 98·8; no cardiac mischief was developed. The urine, scanty and acid on admission, was rendered slightly albuminous from the presence in it of a small quantity of blood. The slight strangury and albumen, however, disappeared in forty-eight hours. He had slept very badly from the commencement of his illness, but as soon as the poultices were applied to the blistered surfaces sleep returned, and was 'good' every night during the time he remained in the Hospital. His appetite, bad on admission, was good on the third day; and his thirst, which was slight when he came under treatment, was not increased by the blisters, is reported to

be absent on the fourth day. The heart was sound when he came under treatment, and free from disease when he left the Hospital.

"*Case* 2.—Wm. P., aged twenty-two, bootmaker, was attacked, for the first time, with rheumatic fever on November 29. He was admitted into the Hospital on December 6, the seventh day of his illness, and was discharged on January 2, 1865. Seven blisters, equal to 133⅝ square inches, were applied on December 9, and the pain which was severe previous to their application, had disappeared as soon as the surfaces had been dressed. The following day, the clinical report states:—'There is no pain anywhere, and, with the exception of a little uneasiness on the right shoulder on December 17, he was quite easy and comfortable until the day of his discharge.' The pulse fell from 110, on admission, to 90 per minute on the third day. The temperature from 101·4 to 98·6. The heart presented a systolic murmur, distinct at admission over base and apex, and was unchanged at the time of the patient's leaving the Hospital. His sleep, which was stated to be bad before the blister application, was improved on the second, good on the third night, and remained satisfactory until he was discharged. The urine presented a slight trace of albumen, as the result of the treatment, but was normal and free from that substance forty-eight hours after the blisters had been applied. The appetite, which was indifferent on admission, is reported to have been 'good' on the fourth day, while the thirst was absent at first, and continued so, unaffected by the local irritation set up by the blisters.

"*Case* 3.—John B., aged nineteen, was admitted on December 8, 1864, the seventh day of his disease, and discharged January 30, 1865. He had, however, been up and dressed from January 11, 1865. Eight blisters were applied on December 9, three on the 12th, one on the 13th, one on the 14th, one on the 21st, making in all 603½ square inches of empl. lyttæ. The day succeeding the application of the eight blisters, the report states, that all pain had disappeared. On the fourth day from that date, the patient has 'now a little pain in the right shoulder, and says, he wants another blister for it,' which is applied and followed by great relief. The clinical account says:—'December 14. The patient has no pain anywhere. December 18—the same. December 19. Had pain in the left knee yesterday; I think he caught cold from going to the closet yesterday. And December 20. The left ankle and knee were swollen. The application of the blister removed all pain and swelling, and never returned during the time he continued under treatment. The pulse fell from 108 to 86, and varied from time to time, with the appearance of the affection in different joints. The temperature fell from 100·8 to 99·4, and also varied with the pulse. The heart presented, before the application, a systolic murmur at the apex, which remained unaltered at the time of his leaving the ward. His sleep, which has been bad since December 2, was pretty good on the night after the blisters had been dressed; moderate on the fourth; good on the fifth; disturbed considerably by a blister on the 6th; good on the following and every succeeding night. The urine.—Its reaction before the blister application was not noted, but the treatment induced some strangury and traces of albumen, and its condition varied from time to time with the variable condition of the joints. Appetite is stated to have been bad on admission; a little better on the second day from the application of the

blisters; improving and gradually becoming good from the fifth day of the treatment. The thirst was slight when the patient entered the Hospital. It appears, however, to have been considerably increased after the application of the blisters, but the symptoms vanished in a few days.'

"*Case* 4.—Charles P., aged twenty, butcher, already previously the subject of acute rheumatism, was admitted on December 8, 1864, the fourteenth day of his illness, and discharged on January 18, 1865. Four blisters, equal to 118¾ square inches, were applied simultaneously, and with such relief that on the day succeeding the application, all pain had disappeared, and his sleep, which for fourteen days had been wretched and without benefit to him, is reported as being good, and continuing satisfactory for the remainder of the time he was under observation. The pulse, which was 80 and 66 respectively on the two days before blistering, became 72, 68, 63, and ultimately 60. The temperature, 100.4° and 98.8° before blistering, became 98.8°, 99.2°, 98°, and ultimately, on cure, 98.4°. The heart was free from disease on admission, and continued the same during the entire period. The urine presented, after blistering, a very slight trace of albumen. The appetite, which had been bad since November 25, was very much improved on the day after the blisters had been applied. The report says, 'he eats everything before him;' and it continued excellent from that date. The thirst was great on admission, only slight after the blisters were applied, and stated to be *nil* on the following day.

"*Case* 5.—Susan J., aged twenty-two, needlewoman, of fair complexion and sanguine temperament, and who had had three previous attacks, was admitted with acute rheumatism, on December 12, the fourth day of her illness, and discharged on February 2, 1865. Six blisters were applied, four on December 12 and two on December 14, epual to 328 square inches. On the day succeeding the first application the report states: 'There is no pain now in the hands and ancles, but pain came on in the night in both shoulders, and she has had a very slight pain in passing urine.' After the second application the report continues: 'Feels much better; no pain anywhere; got out of bed this morning; wrist still swollen and red, but soft, movable and not tender to pressure.' On the following day report states: 'She cannot even invent a pain anywhere; looks well, and is only weak. The pulse fell in this case from 114 to 100, 90 and 70, and was 84 at the time she left the Hospital. The temperature fell from 101.7° to 99.7°, or two degrees. The heart was diseased on admission, and the systolic murmur at base and apex remained unchanged. The urine, at first acid, and containing a slight trace of albumen from the blisters, became alkaline on the second day from the time of the local application, and normal speedily after this date. Her sleep, which had been almost absent from December 8, is stated to have somewhat returned after the blisters were removed, to be much better on the fourth, and very good on the fifth night, and to have continued satisfactory until the time she was discharged. Her appetite, none on admission, was improved immediately after the application of the second set of blisters, and pronounced good on the fifth day after the last named application. The thirst was very great on admission; unaltered the day after the blistering process; much less in amount on the following day, and none on the succeeding days."

[The following case occurred under Dr. Davies' own care at the London Hospital.]

"A case at present under the care of Dr. Davies affords an excellent example of the safe and rapid results of the treatment. The patient was brought into the London Hospital on Friday night, completely crippled from acute rheumatism, and presenting the usual symptoms of that disease. He described his agony as very intense. Ten blisters were at once applied, and a grain of morphia was administered internally.

"On the Saturday evening he was free from all pain.

"On the Sunday he declared himself to be comfortable, and his joints to be movable.

"On the Monday morning he sat up and washed himself, and in the evening was able to walk down the ward to the closet.

"On Tuesday he was perfectly easy, with appetite returned, and thirst absent, and was ordered to have a chop and a pint of Bass' ale, as he had been accustomed to live rather freely. The urine in this case was found to be strongly alkaline on reaction, and perfectly free from any trace of albumen, as tested by heat and nitric acid. He had had no strangury. On being questioned as to the pain induced by the blisters, he said that he would gladly have them repeated were he unfortunate enough to have another attack of rheumatism. No cardiac mischief was developed. No medicine beyond the morphia already mentioned had been administered." — [*Braithwaite's Retrospect, from Medical Times and Gazette, April* 1, 1865, *p.* 333.]

The editor of the Lancet criticises this mode of treatment severely (Oct. 1865, p. 496), his first objection being its inapplicability in private practice. He asks Dr. Davies, "is he sanguine enough to expect that in these hyperæsthetic—not to say homœopathic—days, on which our lot has fallen, such a prescripton will be to any extent feasible in private practice?"

"But the principal objection to Dr. Davies' method is the fact that we have another most unobjectionable and efficient method of treating acute rheumatism —the alkaline method, as practised by Dr. Fuller and others. * * * * It seems to us that there is evidence already to show that acute rheumatism is effectually antagonized by alkalies. Dr. Davies' own treatment increases the alkalinity of the blood, and, indeed, would seem to owe its efficacy to this action. The degree to which the blood is alkalized by the internal administration can be regulated easily; and the administration is itself a procedure so painless and simple that we think this treatment should have a more complete trial before attempting the blistering treatment of a disease which is itself so painful and disabling."

In a subsequent number of the Lancet (Dec., 1865), Dr. Davies defends his practice, adducing cases which have been effectually treated by many blisters, in private practice. Referring to the use of the alkaline treatment, he says:

"I will only quote from my paper the following statement:—'That the blister and alkaline treatments are not incompatible with each other; and I believe a *mild* course of alkali to be a useful adjunct to the directly diminutive blister plan, but not essential, except in those cases where the rheumatic virus attacks the heart in the first instance and appears to hesitate in fixing itself upon the joints.' My object in publishing the results of the plan in fifty cases was to prove that the local treatment, fully and efficiently carried out, is all sufficient in the very large majority of cases to remove the disease rapidly and safely, and without necessitating the internal employment of such large doses of alkali which, while undoubtedly tending to counteract the acid *materies morbi*, considerably impair for some time the general health and tone of the constitution of the patient.

"The blister plan acts quickly, decisively and to the point, at once throwing out the poison, diminishing the temperature of the body, restoring the urine to its normal reaction, and, without interfering in any way with the digestive powers of the alimentary canal, rapidly allows of the return of the appetite. Such a result is cheaply bought at the expense of a little pain."

And in a subsequent note Dr. Davies adds a modifying practical remark, which seems calculated to make the method more popular and practicable:

"I find that good acetum lyttæ—such as can be fully depended upon—well painted into the skin in zones round the affected joints and close to the inflamed parts, produces much less pain than the usual mode of blistering by strips or circular bands of empl. lyttæ. The pain due to the constriction and tension of the strips over the rising surfaces is avoided, and the vesication obtained with but little inconvenience to the patient. The acetum lyttæ must, however, be good; many preparations of it are sold which are utterly worthless. I usually order the skin to be well sponged with warm water, and three successive applications of the acetum lyttæ to be made sufficiently quickly after each other to allow of the certain absorption of the fluid by the surface."

ANATOMY AND PHYSIOLOGY.

1—*The Structure of the Intercarotid Ganglion.*

Dr. Julius Arnold, of Heidelberg, prefaces the account of his investigations on the minute anatomy of the "Ganglion intercaroticum" (*Virchow's Archiv*, xxxiii, *p.* 190) with a historical remark. Although the organ had been described by Haller (1743–'44) Neubauer (1772) and Andersch (1797), and its macroscopical relations had been thoroughly investigated by Valentin, Mayer (1833) and Svitzer (1863), it was Luschka who first published a microscopical analysis of it. The latter considers it as "a glandular organ appended to the cervical portion

of the sympathetic," composed of glandular vesicles, blood-vessels and nerves, and analogous to the "coccygeal gland"; hence he adds it to the class of nerve-glands and calls it "*glandula carotica.*"

In a previous communication Dr. Arnold had shown that the supposed glandular vesicles in the coccygeal gland are in reality vascular formations, so that this body loses its character as a gland. The same is now demonstrated to be true of the intercarotid ganglion. By continued examinations of this body in man (adult and new-born), and in the calf and the dog, A. finds them to consist only of vessels and nerves. The artery feeding the ganglion (arising from the common or external carotid) divides before entering, its two branches commonly sub-dividing again just at their entrance. The ganglion correspondingly is composed of three or four lobules, each of which has a sort of arterial peduncle. These lobules are again composed of a number of bodies similar in form, size and structure. Into each of the latter bodies a canal enters, which sub-divides into a number of analogous formations, inter-communicating and finally uniting to form another or several canals, which leave the body at some other point. Injected preparations furnished the proof that the entering canal is a branch of the afferent artery, so that the canals composing the body are vessels, the body itself a sort of glomerulus.

The structure of the ganglion intercaroticum is stated by the author, after having given detailed descriptions, in these words:

"The lobules composing the intercarotid ganglion consist of bodies which are built up of vascular canals. The latter are arranged after the type of glomeruli, *i. e.*, they represent loops of vessels, which arise from the sub-division of an artery, take the most varied course, anastomose and re-unite to form one or more efferent vessels.

"The nerves of the intercarotid ganglion constitute, on its periphery, between the lobules and the glomeruli, plexus-formations, composed of medullary (*dunkelraudigen*, 'dark-margined') and sympathetic fibres. From the latter, fine threads arise which embrace the canals in the form of a net-work. Ganglionic corpuscles are imbedded in groups into the central points ('*Knotenpunkte*') of the plexus-formations, and are furnished with at least one process; they are therefore polar.

"The connective substance of the ganglion is firm and fibrillary on the periphery, more homogeneous between the glomeruli, and contains in both places cellular elements, with a varying amount of fatty contents."

A. protests against Luschka's designation as "*glandula carotica*," proposing the name of "*glomeruli arteriosi intercarotici*," unless it is preferred to retain the old name of "*ganglion intercaroticum.*"

2—*Experiments on Congelation of Animals.*

"M. Pouchet, of Lyons, has communicated to the French Academy of Sciences a long series of experiments made by him on this subject. The following are his conclusions:

"1. One of the first phenomena produced by the action of cold is the constriction of the capillary vessels, immediately made visible by the microscope. So great is the contraction that no globule of blood can gain admission, so that these vessels remain entirely empty, whence the pallor of frozen parts. 2. The next phenomenon is the changed condition of the globules. The alterations observed are of three kinds. (1) The nucleus quits its envelope and swims freely in the plasma, the free nuclei having a granular appearance and being more opaque than in the normal condition. The envelopes of the nuclei become flaccid and torn, or they are dissolved and disappear. (2) The nucleus still remaining within its envelope has become opaque, and is more or less excentrically situated. (3) The globules may be simply more or less indented at their edges, and of a deeper color. It is especially in the blood of reptiles that the nuclei are found expelled, the globules of mammalia presenting indentations. The number of globules which undergo these alterations and re-enter the circulation is proportionate to the extent of the congelation. When this affects only the limbs, 1-15th or 1-20th only undergoes alteration; but when the animal is totally frozen, almost all the globules become disorganized. 3. Any animal that has been totally frozen, all its blood having become solidified and all the globules disorganized, must be regarded as absolutely dead and beyond any restorative power. 4. When the congelation is partial, any organ absolutely frozen becomes gangrenous and is destroyed. 5. If partial congelation has not extended very far, so that but few altered globules have entered the blood, life is not compromised. 6. But when the congelation proceeds over a great extent, the mass of the changed globules which the thawing throws into the circulation, rapidly kills the animal. 7. A half-frozen animal may live for a long time if we maintain it in that condition, the frozen blood not entering into circulation; but it dies rapidly if we thaw the frozen parts, the altered globules which re-enter the blood rendering it unfit for the maintenance of life. 8. In all cases of congelation, death is due to this change in the blood, and not to the impression made on the nervous system. 9. It results from these facts that the less rapidly frozen parts are thawed, the less rapid is the invasion of the economy by this altered blood, and the greater are the chances of success in the efforts at restoring life."—[*American Journal of the Medical Sciences, January*, 1866, *p.* 238.]

3—*On the Elasticity of Muscles.* By A. P. VAN MANSVELT.

"The investigations on the elasticity of the muscles by Weber, Volkmann, et al., all had reference to muscles of the frog, dissected out or entirely cut off, and these researches could not be applied directly to the muscles of the living body. Donders had already, in 1859, pointed out a method of investigating the elasticity of the flexor muscles of the forearm, which the author has carried out in a series of experiments. This method seeks to determine how much a muscle, which has been brought to a certain degree of contraction, is extended by a given weight.

"The experiments were executed in the following manner: The person experimented upon was seated at the side of a board, so that the humerus was vertical and the axis of the elbow joint immovable, and so that the angle formed by the humerus and the forearm could be read off. The arm was then flexed to any angle desired and a weight attached to the wrist, which, after a short time, usually ten minutes, was suddenly removed by an assistant. The arm then recoiled to an angle which could be read off. As long as the weight hung upon the wrist the flexors of the arm had to sustain the forearm and the weight; when the weight was removed they only sustained the forearm, and therefore were extended less. Their length could in both cases be calculated, as likewise how much weight they sustained in both conditions, from which resulted how much they were lengthened by a given weight and with a given length. In this argumentation it is taken for granted that the recoil of the arm was entirely involuntary; that the contraction was equal before and after the removal of the weight. By exercise this is easily accomplished, and the constant recurrence of the same figures in different series of experiments on different days favors the presumption that the influence of the will was really excluded from these experiments.

"The results of the experiments on muscles *not fatigued* were: the muscle is extended, at least within certain limits, in proportion to the weight. The elasticity of the muscle is nearly equal in different degrees of contraction; a primitive bundle extended by one milligramme increases almost 1 per cent. in length. The *tired* muscle, however, is more extensible than when not fatigued; the will, therefore, must excite it more forcibly to preserve the same length. The influence of fatigue disappears rapidly. The *sensation* of fatigue is no measure of the state of the muscles.—[*Schmidt's Jahrb.*, CXXVI, *p.* 147, *from Nederl. Archief voor Genees-en Natuurk.*]

4—*Deglutition, as observed by Autolaryngoscopy.*

"M. Guinier, in a note presented to the Academy of Sciences, arrives at the following conclusions: 1. Deglutition may be effected without occlusion of the pharynx, by the application of the base of the tongue to the posterior wall. 2. The preliminary application of the epiglottis over the larynx is not necessary during the passage of the morsel of food from the pharynx into the œsophagus. 3. The morsel may come into direct contact with the mucous membrane of the glottis; and the mere contraction of the vocal cords is sufficient to prevent it

from entering the larynx. 4. The mucous membrane of the base of the tongue, of the epiglottis, and of the interior of the larynx, appears to be endowed with a special sensibility; here the contact of food produces no painful sensation, but merely the need of deglutition, while the contact of a foreign body gives rise to cough or attempts at vomiting.

"At the same meeting a note was presented from Dr. Krishaber, in which the following conclusions were given: 1. During the act of deglutition, the alimentary morsel passes into one of the pharyngeal grooves along the side of the epiglottis tilted up by the elevation of the larynx; it thus reaches the œsophagus at the moment when, by the action of its constriction, the pharynx is narrowed. 2. Deglutition of liquids is effected in the same way; but they frequently pass over the epiglottis. 3. A small quantity of liquid comes into contact with the mucous membrane of the larynx and even of the vocal cords. 4. In gurgling, the larynx being widely open, a large quantity escapes into the organ. 5. The alimentary morsel is easily tolerated in the larynx as far as the vocal cords and even in the trachea. 6. The sensibility of the trachea to foreign bodies is much less than that of the larynx. 7. Hard cold bodies—a probe for example—are not tolerated in the air-passages; while soft bodies having a temperature equal to that of the parts with which they come into contact, may remain in the trachea several minutes without producing cough.—[*American Journal of Medical Sciences, January*, 1866, *p.* 238, *from Gazette Médicale de Paris, July* 15, 1865.

SURGERY.

1—*On Peracute Periostitis.*

In the Society of Physicians of Vienna (*Wiener Med. Presse*, 1865, *No.* 26), Dr. Weinlechner—

"made remarks on *peracute periostitis*. It occurs mostly in young individuals, in whom the elevated vitality of the osseous system forms a predisposing cause of inflammation. This disease either arises spontaneously or it is attributed to cold or to a traumatic influence—by a blow, fall, leap—(which, compared to the dangerous consequences, usually appear trifling) as the direct cause. At first the patients complain of a dull or a 'tearing' pain in the region of one of the long bones, seldom of a short, thick bone. For the present only a slight swelling is found, without any particular redness of the skin; but at the same time the general state is disturbed considerably, the patients have violent fever, complain of headache, vertigo, buzzing in the ears, and even delirium sets in, so that the physician who is not sufficiently acquainted with the disease possibly undervalues the local affection, and thinks of incipient typhoid disease, meningitis, or acute rheumatism with exudation into the meninges. In a few days the doubt is cleared up. With the increase of the exudation, the deep-seated swelling grows, extends, shows a high degree of elasticity, finally fluctuation, and the nearer the pus approaches to the skin, the more the latter reddens, becomes œdematous, until at last the pus breaks forth through it. If we now examine with a probe through the natural opening, or if we have aided nature by the formation of an artificial opening so that we can enter and examine with the

finger, we find the periosteum lifted off from the bone, and the latter more or less rough. The spontaneous opening of the subperiosteal abscess requires, in places where the bone is superficial (*e. g.* the tibia), eight to ten days; otherwise it is often delayed four weeks. Hence it is advisable to shorten the process by proper incisions through the skin, fascia and muscles, avoiding the vessels and nerves. In consequence of the separation of the periosteum, the bone, robbed of an essential source of its nutrition, dies, either peripherically or even in its entire thickness. The separation of the dead from the living and the regenerative process initiated by the loosened periosteum, usually keep pace together, and an abnormal mobility at the seat of the affection is therefore rarely observed. But should the process of demarcation proceed more rapidly than the new-formation of bone, *spontaneous fracture* will take place. Such a fracture usually heals before the necrotic bone is expelled or extracted. As in every fracture, a proper position and suitable bandages must be secured during the healing process, or else the osseous capsule will bend and consolidate in that state. Immediately after the opening of the abscess it is impossible to say to what extent the bone will recover, or how much of it will die—even if an accurate examination with the finger is practicable. In the most favorable case recovery will take place after the removal of the necrotic bone, with or without shortening, with or without stiffness of the neighbouring joints. Shortening usually takes place when the bone dies in its entire thickness, and the adjacent joint becomes stiff, if it has taken part in the inflammation, or especially if the pus has found its way into the joint. Many patients, however, succumb in the first weeks to the violent suppurative fever, or they die, after months, even years have elapsed, even if the necrotic bone was removed, in consequence of hectic fever, unless a timely amputation is resorted to to save the life of the patient."

2—*Aural Surgery. The use of the Artificial Membrana Tympani.*

Dr. D. B. St. John Roosa (*Amer. Jour. Med. Sc.*, Jan., 1866, p. 106,) relates in detail five cases in which the artificial membrana tympani was employed; we copy the remarks which he appends to these cases:

"Surgical literature, so far as I can find, has comparatively little reference to cases in which the artificial drum has been worn. I have, therefore, given the foregoing somewhat in detail, in order to show about what may be accomplished by the substitute for the natural membrane. The cases have been taken without any particular choice, from a number of which I have notes. It is the habit of the writer to tentatively apply the artificial membrane to all ancient perforations, where the hyperæmia and inflammation or discharge of pus are not very considerable. Recent cases of perforation, as a rule, heal so readily that the use of the drum is not indicated.

"In order to a successful use of the artificial membrane—

"1. The Eustachian tube must be pervious.

"2. The stapes or incus of the ossicula must be *in situ*.

"3. The inflammatory action of the external auditory canal and remains of the drum must not be excessive.

"It is also of great assistance to the surgeon in procuring a successful wearing of it, that the patient should be intelligent enough to realize, that, at the best, the disk of rubber is a foreign body, which should be carefully removed at any approach of irritation. It is, therefore, not of much use in the case of children, or unusually stupid or careless adults. It should also be stated, that cases have been found where all the above-named conditions have been fulfilled, where it was *à priori* supposed that the artificial membrane would do good, and yet repeated trials proved that the use of it effected nothing for the hearing.

"In these cases, we may perhaps conclude, that there existed very considerable rigidity of the quasi articulation of the stapes with the *fenestra ovalis*."

3—*Dislocation of Cervical Vertebræ.*

From the *Gazette Hebdomadaire* (1865, No. 14) we translate the following item:

"It is with a little incredulity that we collect instances of *traumatic luxations of the cervical vertebræ successfully reduced*. Still there are such as unite all the conditions of authenticity desirable. A fact of this kind was communicated, on the 15th January last, by Prof. Schuh, in the Society of Physicians of Vienna (*Œsterreichische Zeitschrift für praktische Heilkunde*, No. 5, 1865). The case was that of a young girl with whom the dislocation had existed, as it appeared, about a fortnight. It had been produced while the patient, being in bed, had executed a rotating movement of the head. In consequence of this movement the patient had found herself unable to change the position of the head, which remained immovable in an inclined position, the face turned to the right. At the same time violent pain supervened, extending to the face, the right shoulder and the occiput,—pain due to stretching of the cervical plexus, the auricular and occipital nerves. Motion in the upper extremity was at the same time almost completely abolished, and was only partially reëstablished.

"The diagnosis not being doubtful, the patient was chloroformed and reduction proceeded with. Extension was made by means of a cravat passed under the chin. At the same time Schuh, taking the occiput into his hand, gave the head a rotating movement from right to left. The 'choc' of the articular surfaces returning into their normal relations was distinctly heard. The pains which the patient experienced immediately disappeared, and six days later she was able to leave the clinic. Schuh presented this little girl to his colleagues.

Some cases of the same nature were mentioned in the Society on this occasion. Schuh himself reported another case of dislocation by muscular action, which he had observed long ago. The pain radiating to the face, occiput and axilla existed as in this case. At that period surgical anaesthesia had not yet been invented. It was at first attempted to perform the reduction as in the preceding case, the patient being seated in a low chair, but no result was obtained in this position, and the reduction only succeeded after making the patient lie down.

"Dr. Rabl related an analogous case; but in this the luxation had been the consequence of a fall. Rabl, having no educated assistants at disposal, made the patient sit on the floor, and had his shoulders fixed. He made extension by both hands applied on the sides of the head, which was then lead back into its straight position by a gentle movement of rotation.

"In the three preceding cases cerebro-spinal accidents were entirely wanting. It was not altogether the same in the case of a little girl of seven years who was treated by Prof. von Dumreicher. The dislocation had been produced by a blow which the little patient had received on the neck. The head was immovable, forcibly turned to the left and turned backward. The patient was taken with severe pain in the upper extremity, delirium and convulsions. Reduction was effected by the same proceeding as in the case of Schuh, only the head, once returned into its normal position, retained an excessive mobility, and for a fortnight the patient continued to suffer pain and convulsions, owing probably to the stretching of the spinal marrow. At the end of this period, however, consolidation took place, as it appears, without the use of any confining apparatus."

4—*The Diagnosis of Chronic Mammary Tumor.*

"The removal of a scirrhous tumor from the breast of one woman, and of a mammary glandular tumor from another, gave Mr. Paget an opportunity of pointing out some of the broad features which distinguished the two conditions. The glandular tumor, he remarked, was not only very movable, but it seemed to move about freely in the substance of the breast. The scirrhous tumor, on the other hand, could be moved, but not in this way; the breast was carried with it. A long dissection, occupying perhaps three-quarters of an hour, would have been necessary to isolate the scirrhous tumor from its attachments to healthy tissue. The glandular tumor was invested by a complete capsule, so that it was instantly 'shelled' out and rapidly removed. On section, it was strictly glandular in appearance. Placed under the microscope, the elements of proper secreting structure of a gland could be found, without, however, any ducts.

"We noticed that Mr. Paget removed the glandular tumor by a process analogous to that which is often adopted in cases of sebaceous tumors. The mass was lifted, and the integuments, stretched by the fingers and thumb of the left hand compressing its base. A scalpel, with its cutting edge upwards, was then thrust through the base of the swelling, in its long axis, cutting its way upwards through the tumor and integuments. A very few strokes of the knife were then necessary to remove the mass, which was of the size of a walnut."—[*London Lancet, Jan.*, 1866, *p.* 47.]

5—*Remarks on the Operation for Strangulated Hernia.*

After giving a brief statistical report of ten cases operated on by him at St. Bartholomew's Hospital, during the year 1864, Mr. Holmes Coote remarks:

"The only fact of any apparent novelty that I have to communicate is as follows: Thus, in two of the cases of femoral hernia, I succeeded in destroying the stricture by the pressure of the point of the finger, not using the knife at all. This mode of proceeding showed how small an increase in the size of the femoral ring is necessary for the replacement of the intestine; and it also drew my attention more particularly to those supernumerary fibrous bands, internal to Gimbernat's ligament (*i. e.*, nearer the neck of the sac), on the existence of which the irreducibility of the femoral hernia often depends. The femoral ring cannot be readily very greatly enlarged by pressure; hence, when the protrusion is returned, a small ring, of nearly natural dimensions, is left—a condition which exerts a most important influence on the proper adaptation of a truss. My friend Mr. Kingdon, surgeon to the Truss Society, assures me that the very free division and consequent enlargement of the ring, as practiced by some surgeons, renders almost impossible the useful application of a truss."—[*London Lancet, Nov.*, 1865, *p.* 523.]

6—*Hydrocele treated by Pressure after Injection.* By JOHN C. AGNIS, M. B., etc.

"When a hydrocele has been operated on by the injection of iodine, or other stimulating liquid, there remains, after reabsorption of the effused fluid, more or less thickening of the tunica vaginalis and adjacent tissues. The outline of the testicle is obscured, and it remains heavy, hard, and enlarged. I never had an opportunity of ascertaining, by post mortem examination whether the internal structure of the testis itself is in any way altered. It occurred to me that the testicle affected with hydrocele might be more completely restored to the natural state if the enlargement and induration sequent on the injection could be dispersed by pressure applied in the way customary for orchitis. * * *

"My plan is this. Having tapped the hydrocele, and injected a solution of iodine in the usual way, I wait till the tenderness has subsided enough for the patient to bear without pain a degree of manual pressure equal to that which the strapping would produce. Then I apply strapping in the way generally used for orchitis. After this, the swelling sometimes diminishes so fast that the strapping has to be re-applied every second day. I am disposed to think that the earlier the strapping is applied the better, provided the inflammation have subsided enough for it to be done without pain. The safest indication seems to be the degree of pain given by grasping the testicle gently."—[*London Lancet*, August, 1865.]

7—*Aneurism successfully treated by Digital Compression.*

The *Gaz. Hebdomadaire* (1865, No. 29) contains the following case in the reports of sessions of the Société de Chirurgie:

"M. DEMARQUAY presented a patient treated for aneurism of the popliteal artery and cured by digital compression. The tumor, although of recent origin, was eleven centimetres long and ten centimetres wide. On the 18th May, from ten in the morning until midnight, the femoral artery was constantly compressed by ten assistants, relieving each other every ten minutes. The compression was well borne. The work of obliteration and contraction went on so regularly, that, three days later, the aneurism was only five centimetres in length and four in breadth. The only accident which threatened to compromit the cure, and, indeed, endanger the patient, was a broncho-pneumonia, preceded by hæmoptysis. Had the decided arrest of the circulation in the lower limb produced this pulmonary congestion? This is possible; nevertheless the broncho-pneumonia was cured as well as the aneurism. The femoral artery seems to be obliterated as far as the ring of the abductor magnus. At one time, Messrs. Marjolin and Guersant thought that they could feel pulsations in the tumor; but they promptly yielded to the opinion of Messrs. Trélat and Broca, who found that the beating was superficial, and situated in the anastomotica magna. As for the obliteration of the femoral artery, as far as the upper third of the thigh, M. Broca doubts it. The absence of pulsations in an artery does not prove that it is obliterated. A small thread of blood may run through the vessel without producing pulsations in it. In doubtful cases the thermometer can be used as a means of diagnosis. If the elevation of temperature persists in the line of the vessel, it may be considered certain that the current of blood still traverses it."

OPHTHALMOLOGY.

1—*On Strumous Ophthalmia, with Reflex Contraction of the Orbicularis Palpebrarum.*

Mr. ATHOL A. JOHNSON, F. R. S. C., communicates (*London Lancet*, Jan., 1866) a paper on that variety of strumous ophthalmia, (meaning, by this term, "any form of inflammation of the conjunctiva or cornea, occurring in strumous subjects,") "in which *inability* to expose the eye to the air and light, in consequence of spasmodic contraction of the orbicularis palpebrarum, is the primary and characteristic symptom;" this "sypmtom may exist in the most marked degree, with little, or, indeed, with no appearance, of inflammation of any of the ocular structures." He states this contraction to be "palably independent of volition, and of a *reflex* character," excited by the ophthalmic branch of the fifth nerve, rather than by the retina and optic nerve. We copy the author's own resume:

"1. One form of strumous ophthalmia is characterised by *inability* to open the eye, in consequence of *involuntary* contraction of the orbicularis palpebrarum.

"2. This reflex contraction is in relation with hyperæsthesia of the retina or first nerve *alone;* or (more generally) of the ophthalmic branches of the fifth likewise.

"3. The hyperæsthesia of the conjunctival, lachrymal and nasal branches induces the reflex acts of closure of the eyelids, lachrymation and sneezing.

"4. The hyperæsthesia is occasionally uncomplicated; but, in most cases, it is soon followed by low inflammation of the conjunctiva, cornea, etc., dependent possibly on the impaired nutrition of the membrane from the altered nervous action, as well as from the mechanical irritation of the parts.

"5. For the relief of this hyperæsthesia and spasmodic contraction of the lids, such medicines as belladonna, hyoscyamus, conium or strammonium, which allay nervous sensibility and relieve spasm of the sphincters, would seem to be indicated; and of these, belladonna certainly has been found to be practically useful."

2—*On the Affections of the Eye occurring in Cerebro-spinal Meningitis.*

Dr. R. SCHIRMER, of Greifswald, in opposition to Niemeyer, (who, in his essay on the Baden epidemic, speaks of a "keratitis, observed in many cases, beginning with considerable varicosity of the eye and muco-purulent secretion of the conjunctiva, and in a few days resulting in keratomalacia and hypopyon,") thinks that neither the conjunctivitis nor the keratitis can be regarded as immediately connected with the meningitis, since they are observed likewise in other diseases in which the eye is not closed for a long period. A direct dependence on cerebro-spinal meningitis, however, can be asserted of the paralyses of the muscles of the eye, of neuroretinitis and irido-chorioiditis, which the author observed in the former disease. A chorioiditis without iritis has not been observed. The practical inference is, that in some cases of cerebro-spinal meningitis the eye also requires special treatment, consisting chiefly in atropinizing, local blood-letting, and, in case of necessity, iridectomy. (*Klin. Monatsbl f. Augenheilkunde.* Aug., 1865.)

3—*Indications for the Therapeutical Use of Calabar Bean.*

Dr. BENNO RUETE, in an inaugural dissertation on the calabar bean and its application to ophthalmology, gives it as the result of

twenty-six experiments, that the physiological action of calabar is to paralyse the sympathetic, and, very probably, to irritate the oculomotor nerve. The following are his indications for its therapeutical application:

1. Paresis and insufficiency of the ciliary muscle ("m. of accommodation.")

2. Mydriasis in consequence of acute diseases, *e. g.*, after diphtheria, typhus, etc.

3. In those affections of the eye, in which the admission of strong light is not desirable, *e. g.*, retinitis, chorioiditis, etc.

4. In case of transparent maculæ of the cornea, which disturb vision by diffusion of light.

5. In lateral opaque corneal spots, when it is desired to cover them.

6. For the enlargement of the surface of the iris, to facilitate iridectomy.

7. To destroy the effect of atropine, when applied necessarily for the purpose of ophthalmoscopic examination.

8 In recent peripheric perforation of the cornea and prolapsus of the iris, to retract the latter out of the wound.—*Klin. Monatsbl. f. Augenhlk.* July, 1865.)

DENTISTRY.

1—*Galvanocautery in Dentistry.*

The *Gaz. Hebdomadaire* (1865, No. 16) reviews a work on this subject by Dr. J. Bruck (*Die Galvanokaustik in der zahnaerztlichen Praxis. Leipzig*, 1864), and offers the following analysis of its contents:

"The author, having given a detailed description of the galvanocaustic apparatus which he uses, deals of the instruments of variable form which the different operations require.

"These are: 1. Straight galvanocauteries, *i. e.*, platinum wires half an inch in length, for the cauterization of teeth of a single root. 2. Curved cauteries for the destruction of the dental pulp laid bare, and for the cauterization of teeth with more than one root. 3. Wires, furnished at their ends, with a button more or less flattened, for the cauterization of larger surfaces. 4. Spiral cauteries, to desiccate carious dental cavities. 5. Knives, made of thin platinum wire, straight, curved, blunt or pointed, or falciform, to extirpate constricted

('*incarcérées*') growths of the gums. 6. The cutting loop, for the ligation and ablation of fungous growths seated upon the gums.

"The author, in explaining the advantages of the new process, insists, above all, on the equable and intense heat which it produces, and on the facility of regulating the latter by means of those commuting lids ('*convercles commutateurs*') with which Middeldorpf has so ingeniously furnished his apparatus. Thus it is possible, where only a cutting agency is required, to reduce the current to the force of one element, so that the cautery only becomes red hot, while the prompt destruction of parts laid bare would demand the employment of two elements (the platinum wire at a white heat).

"All these instruments can, because of their dimensions, penetrate everywhere; they carry with them not only this—we may say intelligent—heat, but also the light illuminating the parts to be operated upon. The heat is produced only at the point of application and at the desired moment, and the instrument is cold when drawn out—an immense advantage, considering how often, in following the older methods, one had to touch the adjacent parts in this class of operations.

"The cutting loop, finally, destroys all excrescences and polypous vegetations with a pedunculated base, and in all situations accessible to the knife.

"The operation lasts but a few seconds; it is painless, and without hemorrhage.

"The author does not waste time in comparing the new method with the old ones. In applying the red hot iron, the heat is rapidly lost, the soft parts are burned, and the patients alarmed. The potential cautery is very unreliable, easily produces periostitis, and is always injurious to the enamel of the teeth. The torsion of the nerves, lastly, is rather an act of barbarism than a method of operation.

"Galvanocautery is applied with certain success. A. To the osseous parts, of either jaw: 1. To teeth of a single or more roots. 2. To teeth deprived of their crowns, and to aching teeth which do not admit of being filled. 3. To the replacement of simple teeth, when the nerves have been laid bare. 4. To teeth broken off in extracting, with shreds of pulp or nerves laid bare. (It is in this case, above all, that galvanocautery has no equal among the other methods.) 5. For subduing the sensibility of teeth filed or worn. 6. For converting moist caries into the dry. 7. Against caries of the jaw. 8. To arrest hemorrhages.

"B. To the soft parts: 1. To extirpate pedunculated epulis. 2. To cause sloughing in epulis of large base. 3. For the destruction of polypous vegetations. 4. For that of constricted excrescences of the gums. 5. To operate for ranula (M. Amussat has already, in 1853, performed this operation with entire success). 6. To destroy the alveolo-dental membrane denuded at the neck. 7. Against fistulæ of the teeth. 8. Against old cases of parulis and fistulæ of the gums. 9. Against hypertrophy and chronic congestion of the alveolo-dental membrane.

"The author finally adds to this brief exposition of the action of the electric cautery many practical remarks, of which the following is a résumé:

"Galvanocautery, above all, offers the great advantage of economizing vitality, and of enriching conservative surgery, consequently, by a valuable agent.

"Toothache, produced by nerves being denuded, is always cured by this treatment, and the same success attends in cases of periodic toothache.

"To deaden the sensitiveness of teeth which have been filed or worn, the author makes use of various cauteries with buttons and thin cauteries for the interstices. He employs spiral cauteries to desiccate the dental cavities before plugging with gold.

"He arrests bleeding from the gums by cauteries with buttons heated to redness, and hemorrhage from the alveoli after extraction by the curved or straight cautery.

"The alveolo-dental membrane, laid open by atrophy of the gums or resorption of the alveolar margin, loses its sensitiveness when the galvanocautery is laid around the neck and placed in contact with the gums.

"The pain in milk-teeth, with denuded pulp, yields readily to the new cautery. Epulis has been cured by the cutting loop, or by puncture and consequent sloughing. A falciform knife, heated to redness, suffices for the removal of lobular and nodular growths. Hypertrophy and induration of the alveolo-dental membrane have disappeared under the influence of repeated punctures. Chronic parulis, and the fistulæ of the gums resulting from it, have been made to granulate, and cured with facility."

ERRATA.

Page 53, line 7 from bottom, for "*Auscultory*" read *Auscultatory*.
" 62, " 4 " " omit "complete."*
" 76, " 6 " " } for "hydro cephalus" read hydrocephalus.
" 77, " 3 " " }
" 78, " 10 from top, }
" 79, " 15 " " for "predominence" read predominance.
" 80, " 15 from bottom, for "pathagnomonic" read pathognomonie.
" 161, " 11 " " for "the forceps" read craniotomy.

* The word "complete" was introduced inadvertently. It is self-evident, that the compressed space allotted to the department of selections from periodical literature precludes the possibility of even *noticing* all the wealth of the daily contributions to our science; only such works as *Canstatt's Jahresbericht* can attempt a *complete* review, while even such journals as the *British and Foreign Medico-Chirurgical Review*, *Braithwaite's Retrospect*, *Schmidt's Jahrbuecher der gesammten Medicin*,—journals devoted exclusively to the collection and review of the products of medical literature,—cannot claim *completeness*.

Scientific Department.

UNDER THE DIRECTION OF BENJAMIN F. SHUMARD, M. D., ST. LOUIS.

METEOROLOGY OF JANUARY AND FEBRUARY, 1866.

BY GEORGE ENGELMANN, M. D., ST. LOUIS.

	JANUARY.		FEBRUARY.	
	1866.	Average.	1866.	Average.
Barometrical Pressure—				
Mean	29″.690	29″.619	29″.736	29″.589
Highest	30″.510	30″.510	30″.236	30″.479
date	8th.	8th, 1866.	15th.	10th, 1857.
Lowest	28″.974	28″.634	29″.108	28″.659
date	15th.	21st, 1855.	10th.	21st, 1860.
Temperature				
Mean	31°.8	32°.4	32°.6	35°.3
Highest	55°.0	72°.0	69°.0	81°.0
date	29th.	27th, 1864.	27th.	29th, 1840.
Lowest	—1°.5	—22°.5	—9°.0	—15°.0
date	20th.	1st, 1864.	15st.	3d, 1856.
Evaporation	2°.4	2°.6	3°.2	3°.2
Relative Humidity	75.9	73.0	67.9	70.6
Rain and melted snow—				
Quantity	4″.16	2″.14	2″.24	2″ 66
Days on which it fell	7	7	7	8
Thunderstorms	2	1	1	1
Principal Winds	NW. & SE.	W. & SE.	W., then SE.	W., then SE.
Cloudiness	5.5	5.4	4.9	5.2
Fair days	9	10	13	9
Variable days	15	13	9	13
Days without sunshine	7	8	6	6
Stage of River	9½-15¼ feet.	6⅚	6⅔-13⅛	8⅛

As explanations of these tables have been given in the former numbers of this Journal, it is not thought necessary to repeat them here. The reader is referred to Vol. I., p. 52 and 152, Vol. II., p. 152.

January, as well as February, were real winter months, with a higher barometrical pressure and a lower temperature than the average for our region. On January 8th, the barometer attained

an elevation which I had, in thirty years, never observed before. The lowest temperature of the whole winter occurred on February 15th, though the mean temperature of that month was, as usual, higher than that of January.

The quantity of rain, sleet and snow, which fell in January was nearly twice as much as is usual for that month, while in February it was about equal to the average; January was more humid, in February the atmosphere was dryer and clearer than usual.

COMPARATIVE METEOROLOGY OF THE WINTERS AT ST. LOUIS.

	Winter, 1865-6.	Winter, 1864-5.	Winter, Average.
Barometrical Pressure			
Mean	29″.677	29″.585	29″.605
Highest	30″.510	30″.127	30″.510
Date	Jan. 8th.	Dec. 8th.	Jan. 8, 1866.
Lowest	28″.906	28″.769	28″.518
Date	Dec. 11th.	Feb. 25.	Dec. 8, 1855.
Temperature			
Mean	31°.6	31°.6	33°.8
Highest	69°.0	72°.0	81°.0
Date	Feb. 27th.	Dec. 2d.	Feb. 29, 1840.
Lowest	—9°.0	—1°.5	—22°.5
Date	Feb. 15th.	Jan. 26th.	Jan. 1st, 1864.
Evaporation	2°.5	2°.7	2°.8
Relative humidity	74.5	78.9	72.6
Rain—			
Quantity	10″08	7″.84	8″.01
Days on which it fell	20	22	22
Thunderstorms	3	5	2
Principal Winds	SE., then NW.	SE., then NW. & W.	SE. & W.
Cloudiness	5.2	6.0	5.3
Fair days	32	26	30
Variable days	37	40	39
Days without sunshine	21	24	21
Stage of the River—			
Mean, feet	9¾	4½	7
Extremes, feet	5-15	1-15	0-16

The past winter commenced on December 4th and lasted till February 25th, with more steadiness and fewer interruptions than we usually observe in our variable climate, and it thus, to some extent, continued the character of the summer and autumn pre-

ceding, which were also characterized by an uncommon steadiness of their meteorological phenomena.

The above table shows that the barometrical pressure was much higher than the average, and a good deal higher than that of the winter before.

The mean temperature was equal to that of the winter of 1864–5, but by 2.2 degrees lower than the average. Both of the last winters belonged to the nine winters out of thirty-one in which the mean temperature ranged below the freezing point. The coldest winter observed in that almost third of a century was that of 1855-6, with 26.4 degrees; and the mildest that of 1844–5, with 40.4 degrees mean temperature.

Between December 4th and February 25th the temperature ranged above 50 degrees only twice in December, three times in January and three times in February, while it fell below zero once in December, once in January and on three days in February.

The relative humidity was greater than in the winter before, and greater than the average; and so was the quantity of rain and snow, though the number of fair days was greater, and the proportion of cloudiness of the atmosphere smaller. In connection with the greater quantity of rain of last summer, as well as of the present winter, the river was unusually high, though from December 12th to February 21st floating ice more or less covered its surface, except from December 16th to January 12th, when the ice formed a solid bridge over its waters.

The greater coldness of the winter, together with the great humidity, the dry weather often alternating with wet spells and slushy streets, and all this following upon a season of paramount miasmatic character, made the past winter an unusually sickly one. Inflammation of the respiratory organs and of the pharynx, in children as well as adults, rarely of a sthenic character, more commonly of a low type, and catarrhal and typhoid fevers, often of great severity and protracted duration, kept our physicians busy throughout the season. The public health will improve during the next few months, though we may have to look for an accession of vernal intermittents as soon as the first sultry weather of opening spring sets in.

ATMOSPHERIC ELECTRICITY, TEMPERATURE, AND HUMIDITY.

Observations at St. Louis, Missouri, based on daily observations at 6, 9, 12, 3, 6 and 9 o'clock, from morning till night.

BY A. WISLIZENUS, M. D., ST. LOUIS.

I.—*Monthly Mean of Positive Atmospheric Electricity.*

JANUARY.

YEAR	6 A. M.	9 A. M.	12 M.	3 P. M.	6 P. M.	9 P. M.	MEAN OF MONTH	NO. OF THUNDER STORMS.	PREVAILING WINDS.
1861...	15.8	19.4	20.3	15.6	15.2	12.7	16.5	0	SE.
1862...	11.5	15.1	13.2	11.1	11.1	10.5	12.1	0	SE.
1863...	18.2	22.1	18.3	15.0	16.9	10.7	16.9	1	SE. & S.
1864...	18.7	23.4	17.6	13.3	12.3	9.4	15.8	3	SW. SE. W.
1865...	16.8	16.2	11.8	9.9	9.8	9.2	12.2	0	SE. NW. W.
1866...	5.4	6.2	7.8	4.9	5.7	5.6	5.9	2	SE.
Mean.	14.3	17.1	14.8	11.6	11.8	9.7	13.2	.....	.....

FEBRUARY.

1861...	12.8	15.3	13.9	10.5	11.5	8.9	12.1	2	SE.
1862...	16.8	19.1	18.3	14.6	15.2	11.8	16.0	0	SE. & NW.
1863...	18.7	16.4	17.2	14.7	15.8	12.8	15.9	0	SE. E. & W.
1864...	13.4	14.5	12.8	6.4	8.1	12.5	11.3	1	NW. & W.
1865...	12.4	11.0	8.5	7.0	10.3	8.1	9.5	0	W. NE. SE.
1866...	9.0	10.6	7.8	5.9	8.8	6.6	8.1	1	SE.
Mean.	13.8	14.5	13.1	9.8	11.6	10.1	12.3	.....	.....

2.—Monthly Mean of Temperature, Fahrenheit.

JANUARY.

YEAR.	6 A. M.	9 A. M.	12 M.	3 P. M.	6 P. M.	9 P. M	MEAN OF MONTH.
1861	26.9	29.5	35.3	37.4	33.5	30.8	32.2
1862	27.1	27.3	30.1	30.7	29.5	28.5	28.9
1863	31.9	34.0	39.4	41.2	38.3	36.1	36.8
1864	22.6	25.2	32.6	35.1	31.5	28.0	29.2
1865	21.9	24.2	31.5	33.1	30.2	27.9	28.1
1866	27.0	29.1	34.8	36.8	33.8	31.5	32.2
Mean	26.2	28.2	33.9	35.7	32.8	30.5	31.2

FEBRUARY.

1861	34.2	35.4	43.7	46.7	42.8	39.7	40.4
1862	23.9	27.0	33.6	35.9	31.9	29.2	30.2
1863	31.3	33.7	38.7	39.4	36 6	34.4	35.7
1864	30.4	34.4	42.2	45.5	40.9	36.4	38.3
1865	33.4	36.1	41.7	43.3	39.4	36.8	38.4
1866	27.3	30.0	37.0	38.7	35.2	32.5	33.4
Mean	30.1	32.8	39.5	41.6	37.8	34.8	36.1

3.—Monthly Mean of Relative Humidity.

JANUARY.

YEAR	6 A. M.	9 A. M.	12 M.	3 P. M.	6 P. M.	9 P. M.	MEAN OF MONTH.
1861							72.2
1862	91.5	87.5	78.5	79.0	85.2	90.0	85.3
1863	94.1	83.9	75.0	71.9	79.3	86.0	81.7
1864	90.8	79.8	65.9	62.7	75.4	79.8	75.6
1865	89.3	77.5	64.3	62.8	73.7	79 9	74.6
1866	87.1	77.1	67.2	65.3	74.6	79.1	75.1
Mean	90.6	81.1	70.2	68.3	77.6	82.9	77.4

FEBRUARY.

1861							63.3
1862	89.1	79.2	65.8	61.5	72.2	75.5	78.9
1863	94.1	83.9	75.0	71.9	79.3	86.0	81.7
1864	79.0	65.0	53.5	49.0	59.9	70.0	62.7
1865	86.2	73.8	63.7	60.8	70.8	76.7	72.0
1866	85.3	70.8	62.9	59.5	68.6	76.5	70.6
Mean	86.8	74.5	64.2	60.5	70.2	76.9	70.7

Errata. In the last No. of *Journal*, on page 70, line 6, for the word "*destination*" read *declination*.

Editorial Department.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

PAGET'S SURGICAL PATHOLOGY.

[Continued.]

Mr. Paget embraces in his theory the doctrine of complemental nutrition, first taught by C. F. Wolff, illustrating the subject as follows:

"A great change in nutrition rarely takes place in a single organ at a time; for example, the growth of the beard at the period of puberty in man, the growth and perfection of the plumage of the bird at breeding time; but as in man, when the development of the genital organs is prevented, that of the beard and all other external sexual characters is, as a consequence, hindered, so in birds, when the breeding season ends, and the sexual organs pass gradually into their periodic atrophy, at once the plumage begins to assume the pale and more sober colors, which characterizes the barrenness of winter." He next refers to certain interesting specimens presented to the museum by Sir Philip Egerton, showing the interesting fact, "that if a buck be castrated while his antlers are still covered with velt, their growth is checked, they remain as if truncated, and irregular nodules of bone project from their surfaces." "The fact is not, hitherto, explained; it is inexplicable, by believing that the materials which, in the formation of these organs of external sexual character, are removed from the blood, leave or maintain the blood in the state necessary for the further development, growth and active function of the proper sexual or reproductive organs." "The concurrent development of the thymus gland and air breathing organs during the body's growth of the thyroid gland and the brain, (instances

of commensurate development cited by Dr. Simon,) of the spleen and panecreas, (as pointed out by Professor Owen,) and I would add of the mammary gland; for the same theory—that each organ of the body, while it nourishes itself, is in the character of an excretion to all the rest—may hold true concerning the formation of certain organs which are, finally, connected in their external function." "If in health, there is such a thing as complemental nutrition, it must, in disease, be the source of many disturbances —through definite change in the blood—between parts which are not specially connected through the nervous system."

Mr. Paget, we think, lays too much stress on the connection between the development of the appendages of the skin and the perfection of the sexual organs. Puberty is marked, not only by changes of superficial structures, but the bones as well undergo remarkable alterations. The pelvis in the adult female is no longer what it was in childhood; before puberty, its dimentions correspond to those of the male pelvis, the brim being contracted and more or less triangular, the antero-posterior measurement equalling or even exceeding the transverse. There is increased development and symmetry of the limbs and general outline of the body, an alteration of the physiognomy and a striking change in the character of the voice, which acquires a deep tone, very different from that of boyhood. This alteration of the voice does not take place in eunuchs, who retain throughout life a shrill tone, of higher pitch, approximating more in character to the female voice than to that of the male. In short, puberty is marked by a visible alteration of the system at large, which indicates the complete development of the entire organism.

The second condition enumerated as essential to healthy nutrition is a *regular supply* of blood, in or near the part to be nourished. There is a general misapprehension respecting the the relations of the blood vessels to the tissues, and, consequently, a misinterpretation of the changes which present themselves in disease. Fat cells, nerve cells, muscular fibre and nerve tubes are as distinct from the system of capillary vessels as are the hair, nails and epithelial cells; the ultimate elements of all the

tissues are alike extra-vascular. There is, probably a system of anastomosing elements, *canaliculated*, forming an appendage to the large system of the blood and lymphatic vessels of the soft structures identical in all respects with those of the bones. All growing parts imbibe their nutrient materials from the nearest source of supply. The so called vascular tissues differing from the non-vascular, as to their relation to blood-vessels, only in this, that the former are in nearer proximity to them than the latter. The osseous tissue, says Mr. Paget, is vascular or non-vascular, according to the thickness of its layers; for when it forms only a thin lamina, such as that of the lacrymal and turbinated bones in man, or of the scalpula in a mouse, the tissue can draw sufficient nutriment from the vascular membrane in contact with its surface. The islets of solid bone encircled by the Haversian canals, are really as non-vascular as are the thin lamina which are not penetrated by those channels. It is only by acquiring clear ideas on this point, that we can understand the true relations between the circulation of the blood and the nutritive processes. The blood-vessels have no more direct connection with nutrition than have the water pipes which supply a large city with the diverse applications made by its diverse inhabitants, of the liquid they convey. We may thus avoid the error of speaking of the action of vessels, "as if the vessels really made and unmade the parts, perverted or restored their normal actions."

The third condition of nutrition dwelt on by Mr. Paget is *a certain condition of the nervous system.* There are numerous phenomena which show that the formative processes are capable of being greatly modified by the nervous system, yet there can be no question as to the independent vitality of the elementary parts of the several tissues, and that the elements can be excited to action without any influence or modification of the nervous structure. Nay, more; a gland can be excited by certain substances to increased action, even after all nerves belonging to the part have been cut off, establishing the fact that irritability is a property not of the nerves alone, but of all the tissues. The influence of the mind over the nutrient process is well known. Mr Paget gives a

good illustration of this principle, viz., the case of a tumor in the breast, which had sprung up under the apprehension of cancer, and disappeared as a consequence of an assurance, on his part, the disease was simple and would go away. The influence of mental emotions upon the processes of secretion are still more unequivocal. The mammary secretion in a nursing female is augmented whenever the idea of the infant is strongly brought before the mind; again, strong emotions of a depressing kind may so alter the quality of the milk as to render it unwholesome. "A lady," says Mr. Paget, "who is subject to attacks of what are called nervous headache, always finds, next morning, that some patches of her hair are white, as if powdered with starch. The change is effected in a night, and, in a few days after, the hair generally regains its dark brown color."

It is an interesting question to determine what class of nerves most influence the nutrient processes. The effects of the distruction of the trigeminal nerve, while the molar nerves of the parts which it supplies are unimpaired, prove that a more direct influence is exercised, through sensitive or sympathetic nerves. "Some recent experiments of Snellen would appear, indeed, to show, that after division of the trigemenal nerve, destruction of the eye-ball may be, for a time at least, averted, by preventing all irritant from coming in contact with the front of the globe. This Snellen effected by closing the lids, and stitching the sensitive ear of the rabbit he operated on in front of the eye. Ten days after the operation, the eye-ball was clear and normal. The conclusion he draws is, that section of the trigeninus does not of itself produce keratitis, nor affect the nutrition of the globe. Schiff has carefully repeated the experiments of Snellen, and find that in every case after section of the nerve, hyperæmia of the iris and conjunctiva immediately occurs, followed by more or less opacity of the cornea, and that the slightest irritation will produce inflammation and destruction of the eye. Whilst the above experiments show that section of the nerve, involving withdrawal of nervous influence, may produce but little change in the mode of nutrition of an organ, beyond hyperæmia and the conse-

quences resulting from it, other experiments illustrate the very serious mischief which may arise if a nerve be irritated. Thus Samuel, by passing a galvanic current through the Gassessian ganglio produced immediate vascularity of the conjunctiva, an abundant flow of tears and lncreased sensibility of the parts, which, after a time, were followed by violent inflammation with opacity of the cornea." The experiments of Bernard, Wallen, Schiff, Wharton, Jones and Lister have shown that the nervous system exercises a most important influence over the condition of the blood-vessels of a part; they being enlarged after the division of the nerves, and again contracted, when, after division, galvanism is applied. Lister has shown, that the controling power is not exercised exclusively by any single part of the nervous axis, but that the function is apparently regulated by the whole length of the cord and the posterior part of the brain. His experiments would also render it probable that there is, in addition, a co-ordinating ganglionic apparatus in the part itself, which, under special conditions of direct irritation, is capable of independent action. Of great value, also, in their relations to this important subject, are recent observations of Lister on the cutaneous pigmentary system of the frog. He points out that the pigment is contained in radiated cells, and that the light or dark color of the frog's skin depends upon whether the pigment be concentrated in the centre of these cells or diffused throughout their processes. These pigment cells are under the control of the nervous system; for when the nerves, going to a part, are divided, a diffusion of the pigment and a dark tint of the skin, comparable to the arterial relaxation, which takes place from the same cause, are produced."

The last condition mentiond as essential to healthy nutrition, *is a healthy state of the part to be nourished.* "This is, indeed," says Mr. Paget, "involved in the very idea of assimulation, wherein the materials are supposed to be made like to the structures among which they are deposited; for unless the type be good, the antitype cannot be." In a part which was originally well formed and with the three conditions of nutrition already

illustrated have been always present, the fourth condition will be, probably, never wanting, for the part will not of itself deflect from the normal state.

Now that our author has disposed of the blood and nerves as conditions of nutrition, he must now refer to the elementary forms which make up the tissues, including blood-vessels, nerves, and in short constitutes the parenchyma of all the organs. The elementary particles of the several tissues he recognizes—not without some inconsistency of expression—as the sole and essential agents of the formative processes. "The new materials," he says, "are supposed to be made like to the structures among which they are deposited, for unless the type be good the antitype cannot be," and as he elsewhere teaches, that in the mutations of the more internal parts, as the muscles, "there is not such a bodily replacement as in the structures more superficially placed, but that the nutritive changes partake more of the molecular character, one particle being replaced by another, whilst, as it were, the original skeleton or frame-work of the texture is preserved." If the new material does not displace bodily the solid forms which go to make up the organs, then such material must be assimilated to those forms and by their agency; for certainly the blood and nerves only subserve the purposes of the "cytoblasts and nuclei." "These nuclei," to use the exact words of Mr. Paget (such as are seen so abundantly in strong, active muscles), "are not the loitering, impotent remnants of the embryonic tissues, but apparatus of power for new formation."

It follows, then, that "the exactness of assimulation, as accomplished by the formative process—"that process by which, through life, certain characteristic differences, even in the several parts of one organ, preserving, also, all those peculiarities of structure and of action which form the proper features, and indicate the temperament of the individual"—are compassed by the immediate agency of the elementary forms of the tissues. Mr. Paget continues: "When any part or any constituent of the blood has been injured or diseased, its unhealthy state will interfere with its nutrition long after the immediate effects of the injury

or disease have passed away. Just as in healthy parts the formative process exactly assimulates the new materials to the old, so does it in diseased parts: the new-formed blood and tissues take the likeness of the old ones in all their pecularities, whether normal or abnormal; and hence the healthy state of the part to be nourished may be said to be essential to the healthy process of nutrition. "For after any injury or disease by which the structure of a part is impaired, we find the altered structure—whether an induration, a cicatrix, or any other—as it were perpetuated by assimulation."

The attentive reader will observe that Mr. Paget's memory does not always serve him, for the remark in the above abstract, that the three conditions of nutrition already illustrated being present, the fourth will probably never be wanting, is strikingly irreconcilable with the declaration in the first lecture, "that the blood is often engaged in correcting the errors to which the tissues, more than itself, are subject." Again, Mr. Paget's elucidation of the exactness of the formative processes goes far to establish the independence of the tissues, establishing almost conclusively that they are the laborators of their own substance. Admit the doctrine of complemental nutrition as taught by our author, and the loss of the smallest portion of any of the organs or tissues would involve the necessity of an immediate readjustment of the blood, in order ro restore the precise adaptation insisted on by Mr. Paget, for the organic compounds elaborated by the blood for the lost part must be retained, thereby disturbing the nice relationship between the blood and the solid structure, and an additional supply of material must be formed for the tissue of the scar, which cannot feed upon the compounds made for the original structure. If so, the "mark made upon the tissues" would soon wear out. But such is not the case, for the scar grows as the body grows, "so that a scar which the child might have said was as long as his forefinger, will still be as long as his forefinger when he grows to be a man."

"The maintenance of morbid structures is so familiar a fact that not only its wonder, but its significance seem to be over-

looked; for this exactness by which the formative process in a part, maintains the change once produced by disease, offers a reasonable explanation of the fact, that certain diseases usually occur only once in the same body. The poison of small-pox, being once inserted, soon, by multiplication or otherwise, affects the whole blood; alters its whole composition; and, finally, the blood recovers to all appearance its former state. Yet it is not as it was; for now the same material will not produce the same effect, and the alteration thus made in the blood or the tissues is made once for all; for commonly, through life, the formative process assimilates and never deviates from the altered type, but reproduces materials exactly like those altered by the disease; the new ones, therefore, like the old, are incapable of alteration by the same poison, and the individual is safe from the danger of infection. After the vacine and other inoculable diseases, it is most probable, not the tissues alone, but the blood as much or more than they, in which the altered state is maintained; and, in many cases, it would seem that, whatever materials are added to the blood, the stamp once impressed by one of these specific diseases is retained; the blood, by its own formative power, exactly assimilating to itself, its altered self, the materials derived from the food."

In taking leave of Mr. Paget, we thank him most cordially for the knowledge and pleasure he has afforded us, and express the hope that, at some future time, we may be able to study other parts of his invalvable work with as much attention as we have given to the portions we have passed in review. The work embraces most subjects of interest to the surgical pathologist, and Mr. Paget has evinced his usual philosophical sagacity in the disposition of them. We do not hesitate to pronounce it the most able and comprehensive work on surgical pathology in the English language. G.

MISCELLANY.

ANNUAL COMMENCEMENT OF THE ST. LOUIS MEDICAL COLLEGE.

The Commencement Exercises of the present year in this Institution, held before a densely crowded and respectable audience, in the large hall of the College, on the night of March 2d, were more than usually interesting.

The presiding officer, Professor John T. Hodgen, M. D , Dean of the Faculty, announced the names of the successful candidates for graduation, to whom Professor Jno. B. Johnson, M. D., presented the diploma of the College, with the following appropriate address:

GENTLEMEN:—By the recommendation of the Faculty, the Trustees have granted you this diploma; and I have been chosen, on the part of the Faculty, to present it to you, and to convey to each one of you their earnest wishes for your success, as regards the healing art, through that future of years which lies before you. In receiving this diploma it should be with a true valuation of the rights and privileges it confers upon you, and you should remember that it is incumbent on you to return to the profession a part of that information which you have received. We believe that this is your determination.

This diploma conferred on you by the Trustees is a simple piece of parchment, setting forth your qualifications as Physicians. It is nothing to you, only so far as your conduct in the future, as men and physicians, will prove you worthy of the confidence shown to you in its presentation. The mere possession only speaks of the past and present: the future is unwritten; it belongs to you; but that you will so act as to show yourselves worthy recipients of the honor conferred, we have full confidence.

In parting with you, I do not know what better to say, or what better advice to give, than in the pursuit of your profession to have due regard for your own efforts, rather than too much reliance on books. Heretofore, books have been your chief instructors; now, that you enter upon the practice of the art, you must

judge for yourselves of the truth of the authority which you consult. You must rely on your own judgment and observation. We have confidence you will not betray the trust reposed in you; we believe that each one will act up to the requirements demanded of him, and will remember that *improvement* is a duty which you owe not only to yourselves, individually, but to the profession.

After the address of Professor Johnson, the following valedictory address was delivered to the graduates, by Dr. Warren B. Outten, one of the graduating class:

FRIENDS, FELLOW STUDENTS:—Brief has been the space since first we assembled together, quickly has the swift foot of time brought about the hour so eagerly desired, and we have gained the consummation of our hopes, so devoutly wished and so earnestly worked for. Now, as the hour of parting draws near, and the ambition, for a moment, lulled into rest by the thought of parting, we, for the first time in our studentship, feel the pang of regret, for many have been the hours of true pleasure that we have passed together; bright and profitable have been the halcyon days of college labors. Like a soothing and pleasant dream, they have gently run their course, and left painted in vivid tints the pictures of many happy, and aye, too, of some perplexing moments. Oft in after years, when the snow of time shall have whitened your locks, and you will have become honored and venerated in the study and practice of your noble profession, will you think of the happy days gone by. Oft will you pleasantly think of your Alma Mater and her professors; oft will your musing soul bring back the remembrances of past collegiate days and associates. Companions, friends, you have chosen a noble profession —a profession of which you may well be proud, for there is none nobler, none whose aim is higher or purer, and one whose glory, aims and labors are for the good of man. 'Tis not brilliant nor fascinating to the romantic world, like that of war, for 'tis free from passion and liberal of good; nor is it like stern, dignified and unyielding law, but 'tis kind, humane and merciful.

Its quality of mercy is not strained,
But droppeth as the gentle dew from heaven.

To the true friend of humanity 'tis sublime, 'tis grand in its achievements and endeavors. Medicine is a quiet profession, though the world be dissentious; it ever is working on with its glorious aim in peace; never, by intestine strife, has it caused the blood of innocent thousands to be shed; never, by its teachings, instilled within the soul of man, revengeful hate; but teaches gentle love and philanthropy. War was born of jealousy and hate, but it from sympathy and love. It follows in the path of rough, unpitying war, soothing his ruthless touch and unraveling the imperfect work of this cruel god. It was born with man; it sprang forth from the soil of pity like a delicate plant oft shriveled by the hot, burning sun of ignorance, but it was nourished into a grand and glorious existence by the tender and ever watchful. Look back unto the days of its infancy, its workings were enshrouded, mixed with ignorance and mystery. Slow, indeed, was the progress of our art in comparison to others in Greece; it dragged its slow length along until the time of Hippocrates, who was the true stamp of a great man—with him it received its first great advancement. It was he that commenced the glorious work of making it into the mighty world of science that it is now. He did for it all that one man could do; yet, e'en in his day, it was looked upon as scarce worthy of holding a place among the arts. After his time, it slowly but surely improved its condition; but when the bright star of Greece's glory waned and faded, it fled from thence and continued its onward progress with the advance of enlightenment. Yet, still was it distorted by the ungainly features of deep ignorance, when, in the arena of Rome's golden age, Galen, its hero, appeared to brush away the dusk of ignorance and mystery which enshrouded it, and raised it to the acme of then its pristine glory. Yes, the enlightened world was proud of this bright genius—before his transcendant intellect faded away the mists of gross empiricism. Imperfect as his views were, yet inconceivable was the good derived from them; much does medicine owe to this great man, from him it went forth to the

then known world respected, as it was never before. Onward was its course, until the demon discord rent in twain the noble and mighty heart of Rome, and she was stricken and trampled under foot by the barbarian; then medicine, for a moment stunned, fell with her sister sciences, engulphed in the general ruin. Severe was the shock, yet she arose from amidst the wreck of her once great and glorious home, and followed in the path of enlightenment. Mighty were her struggles; through long ages did she work incessantly; brighter and brighter grew the star of her glory, e'en though oft spurned and cast aside; but as her Stahls, her Hoffmans, her Cullens and her Ambroise Parés appeared upon the stage, quickly did her glory augment and her enemies sink into oblivion. So now she stands in her sublime and beautiful glory, built upon the firm foundation; and like a mighty Colossus doth bestride the world. Yes, e'en from a tiny spark, it has spread into a universal flame, which casts its benign glow upon the civilized earth. Onward, ever onward, is her course; her votaries, once a feeble few, but now mighty hosts. Companions! we have but commenced the work which is before us; let us not foolishly and thoughtlessly imagine, because we have gained medicine's first gift of merit, that we now can build castles in the air of a prosperous future. No, let us work with unceasing energy. A physician's life is not one of ease and idleness; it is one of labor, and if we would be men in our profession, why then we must labor, and in laboring we will be happy, for the sequel of all true happiness is employment. Let us work, then, and be true to our profession, and we will be respected in our communities. Go where you will, and you will find the true physician ever at his post of honor, loved and respected for his labors. Go to the city, and there you will find him laboring in medicine's temples, and adding to its ranks the rising men; and, amidst the many thousands which crowd its thoroughfares, will you see the physician hurrying on in his good work; and in the deep, dark and lonely night, when its streets are deserted and changed from the noisy clang of busy life, with gloomy, thoughtful silence will you hear the physician, as o'er the rough streets

he speeds on in his errand of mercy. In the country, you will find him there, likewise; a man in the midst of thunder, lightning and rain, and in the chilly blasts of the hardy north wind and snow he may be seen on horse, dashing to the place of sorrow and distress, and in his heart there is a strengthening glow which thus makes him defy the elements: it is the consciousness of doing his duty towards God and his fellow man. On the battle-field, amidst the awful booming of great guns and the incessant clatter of firing musketry, the infuriated yells of the opposing thousands, the torturing and distressing groan of the wounded, the agonizing shriek of the dying, the nervous and revengeful whirl of war's destructive missiles, and the thick clouds of sulphurous smoke, he may be seen soothing the pains of the wounded and chafing the brow of the dying. Many an earnest and heartfelt "God bless you" from the suffering man greets his ears, and blesses him in his good work. When grim pestilence, who has laid dormant in her caves, and increased her bitterness, rises up and cries havoc, and lets loose her direful hosts, who strike down alike the earth's great and low; its rising, its beautiful, its aged and unpretending flowers, and leaves tears of misery in her path, from the fatherless, the motherless, the widowed and childless, making this earth a very chaos of woe—then may he be seen battling against his strong and horrid foe, oft discouraged, but never despairing; and if he too becomes a victim, why then he falls with the glorious conviction of nobly doing his duty, and dying gloriously in the cause of humanity. Is there not glory in such deeds, e'en though they are not like the deeds of some ambitious ruler, who has made the earth crimson with innocent blood, emblazoned on the replete and oft too false page of history? What can be nobler than his aims and endeavors to save the life of man, his fellow creature, at the hazard of his own? Why, companions, it is a glorious thing: the cause of humanity alone should spur us on with redoubled efforts. Would you rather drag slowly and lazily along in this busy world of ours, without aiming at a brighter star than pure egotism, or would you rather set high your aims and labors, and thereby win a glorious name

and ease the sufferings of the pained millions? What true friend of humanity would not rather be a Jenner than a King Alexander? Jenner has saved men, but Alexander destroyed them. Jenner, ever man's benefactor; Alexander, never, but once his scourge. Who knows what Jenner or Bronco did? Few outside of the medical profession; and yet the romantic world has Alexander's deeds by heart. O, how happy and peaceful must the thoughts of that man be who receives the earnest and sincere thanks from a grateful world for his kind and humane labors! Think, companions, of the good that you can do. Oft can you ease the pangs and brighten the hopes of those who dwell with poverty and misery, and receive—not thanks which are uttered by cold formality's voice, but thanks brimfull of sincerity and gratitude—oft by your labors dissipate the tears of sorrow and bring smiling happiness back again, make sunshine illumine a painful darkness, and do a very world of good. And when this, thy earthly frame shall be eaten away by cankering, and no longer fit to hold its immortal spark, and dissolution draws near, then will the past deeds of good illumine the parting hours of thy soul, ere it forever bid farewell to earth.

Now, companions, fellow graduates, let us be ambitious in our studies; work, strive, toil on, and aim to know well our profession; we cannot learn it in a day, nor in a year, but takes long years of labor. Excellence is only to be attained by hard work, it does not spring forth at the mere whim of man. Let us never be discouraged, storm every impediment with energy, perseverance and hope, for "labor omnia vincit." Look upon the vast fields of learning which medicine now presents, think you that one feeble mind can be perfect in all its branches? No, you cannot think so; therefore, let us not strive to be perfect in all, for if we do, we will be perfect in none. Let us take that branch which presents to us the most fascinating features, search well into the deep and dark cave of obscurity, and bring forth to the world the brilliant hidden gems of wisdom which pertain to it. Medicine is a field open for talent, in it merit will have sure favor, and industry its due reward. Let us not start out too sanguine and

imagine, like hopeful and inexperienced youth, that our paths will be strewn with flowers; no, let us prepare for every emergency and difficulty, and success will come. Again, I say, labor, and we do. Why should not the Saint Louis Medical College produce a Cooper, a Velpeau or a Mott? Surely the rising West hath given to the world some as bright and brilliant stars as e'er shown on the horizon of fame. Let us be observant and mark well all that may come in our way, and aim to win distinction in one, the most dignified and profound of human occupations. And now, companions, farewell; be just and fear not: let all the ends thou aimest at be thy country's, thy God's and truth's. May your dearest hopes be realized; happy, prosperous, peaceful be your days; may your hours of labor be bright and happy; may misfortune be a stranger to you and yours, and you be honored for your goodness and worth, is the sincere wish of your friend and fellow student. To you, kind professors, ere we say farewell, a word from the class. Much do we owe you for the kind interests that you have taken in our welfare; you have taught us a goodly path, and by your works have shown us what can be gained by labor; through long years will your memories be ever pleasant and fragrant to our minds. Receive our earnest and heartfelt thanks. We wish you the happiest of days. May your path through life be strewn with flowers of happiness and joy, and may kindly fortune guide you to prosperity's height, is the sincere wish of your class.

The valedictory of the College was given by Professor Chas. W. Stevens, M. D., which we regret we were not able to report for publication in the present number of the *Journal*. It was a practical and able lecture on the duties and relations of the Doctor of Medicine.

The present Session of this College is a great success over any previous year, the number of students being one hundred and eighty, and the graduates fifty, viz.:

Chester Adams, Fractures; Maurice Andre, The Doctor; Jos. B. Bishop, Histology; John B. Blain, Dysentery; John B. Bond, Chloroform; Wm. A. Brock, Encephalitis; D. L. S. Bland, Animal Heat; Edwd. B. Bowles,

Pneumonia; Geo. R. Bowles, The Blood; Frederick Benckemann, Cerebro-Spinal Meningitis; Henry M. Chevins, Intermittent Fever; Edwd. E. Coleman, Etiology; Geo. G. Duggins, De Sanguine; Francis O. Drake, Menstruation; Gustus F. Eimbeck, Reception of Mercury in the System; Wm. G. Fisher, Thanatology; James A. Goslin, Disease and its Causes; Wm. H. Gasney, Intermittent Fever; Chas. F. Goodrich, The Blood; Thos. C. Hussey, The Physician; Lemuel T. Hall, Typhoid Fever; Arthur Hazzlewood, Malaria; Simeon P. Ives, Typhoid Fever; Jacob Keller, Water; Willis P. King, Abortion; David B. Limberger, Respiration; Leonard G. Loring, Signs of Pregnancy; Henry C. Lindsay, Function of Blood; Andrew W. McAlester, Urology; Wm. H. Middleton, Oxygen; Wm. W. Macfarlane, the Country Doctor; Lemuel S. Mead, Pneumonia; Nerman B. Michael, Dysentery; Henry H. Mudd, the Formative Process; Benjamin B. Minor, The Glycogenio-function Liver; William Nifon, Puerperal Convulsions; Francis M. O'Daniel, Malaria; Warren B. Oulten, The Production and Elimination of CO 2; Ferdinand B. Philpott, Puerperal Peritonitis; Ralph A. Quarles, The Action of Diuretics; Columbus McDonough Ross, Diphtheria; Thos. B. Ross, Auscultation and Percussion; Thos. B. Toalson, Uterine Hemorrhage; Nathan D. Tirrel, A few Thoughts about Cholera; John Wilson, Jr, Importance of a thorough Knowledge of Anatomy; Felix M. Woodfin, Fractures Simple; Caspar A. Weber, Cutaneous Excretions; Frederick Wm. Wesseler, Typhoid Fever; Thos. J. White, Physiology; Thos. J. Wright, Puerperal Peritonitis.

Upon the following gentlemen the Ad Eundem Degree of Doctor of Medicine was conferred, viz:

Levinus Dunham, Matthew Burchard, Isaac Folsom, Oscar F. Potter.

The Honorary Degree of Doctor of Medicine was conferred on Dr. John B. Howard, St. Joseph, Mo.

The Annual Meeting of the St. Louis College of Pharmacy, will be held on the Evening of March 26th, at 7 o'clock, at the College Rooms, No. 19 Fourth Street, 3d. Floor. There will be, no doubt, a full attendance of all the members, as business of great importance will be transacted.

The first number of the *Journal* was mailed to all our subscribers, with a bill for the present volume, to which nearly all have responded with renewed subscriptions. Our patrons have our thanks for their cordial support.

DR. MONTROSE A PALLEN.

We omitted to mention, in our last, that our fellow townsman, Dr. Montrose A. Pallen, had returned to this city. It will be remembered, that, before the Military Commission which was sitting in Washington City, in June last, his name was mentioned as concerned in a charge of a desire to poison the Croton River Reservoir. The charge was made by one Sandford Conover, *alias* James Watson Wallace, etc. The Government, having fully investigated the matter, and being satisfied of the innocence of Dr. Pallen, granted him a full pardon. We hope the American Medical Association, for its own credit, as well as for justice to its members, will, at the next session, promptly recall its past hasty action, and award to Dr. Pallen a full acquittal.

We have a number of valuable books to be noticed, which, owing to the great press on our time and space, we have not yet reviewed. In subsequent numbers we will give them their due and proper notices. Among which are; per Keith & Woods:

THE PRACTICE OF MEDICINE. By THOMAS HAWKES TANNER, M. D., F. R. S. From Fifth London Edition. Philadelphia: Lindsay & Blakiston. 1866.

CHLOROFORM, ITS ACTION AND ADMINISTRATION. By ARTHUR ERNEST SANSOM, M. B. London. Philadelphia: Lindsay & Blakiston. 1866.

LECTURES ON EPILEPSY, PAIN, PARALYSIS, AND CERTAIN OTHER DISORDERS OF THE NERVOUS SYSTEM. By CHARLES BLAND RADCLIFFE, M. D. Philadelphia: Lindsay & Blakiston. 1866.

AN ADDRESS IN RELATION TO EPIZOOTIC DISEASE AMONG SWINE, IMPROPERLY CALLED "HOG CHOLERA."

COMMUNICATIONS OF THE RHODE ISLAND MEDICAL SOCIETY, FOR THE YEARS 1860, 1861 and 1862.

HEREDITARY TRANSMISSION; AN ADDRESS. By B. LINCOLN RAY, M. D.

NOTICE.

Patrons having duplicate copies of Numbers 1 and 2 of 1865, and No. 1, 1866, that they may not wish to preserve, will do us a favor by sending to us at our cost, and will be credited with the usual subscription price.

Our readers will observe that we have added sixteen pages to the present issue. This was for our convenience in the pressure of matter, and will not be uniform.

AMERICAN MEDICAL ASSOCIATION.

The Seventeenth Annual Session will be held in the city of Baltimore, Tuesday, May 1, 1866.

The following Committees are expected to report:

On Prize Essays; Dr. Austin Flint, Sr., N. Y., Chairman.
On Quarantine: Dr. Wilson Jewell, Pa., Chairman.
On So-called Spotted Fever; Dr. Jas. J. Levick, Pa., Chairman.
On Ligature of the Subclavian Artery; Dr. Willard Parker, N. Y., Chairman.
On Tracheotomy in Membranous Croup; Dr. Alex. N. Dougherty, N. J., Chairman.
On Rank of Medical Corps in the Army; Dr. C. S. Tripler, U. S. A., Chairman.
On Rank of Medical Corps in the Navy; Dr. T. L. Smith, N. Y., Chairman.
On Medical Literature; Dr, C. A. Lee, N. Y., Chairman.
On Medical Education; Dr. Samuel D. Gross, Pa., Chairman.
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WM. B. ATKINSON,
Permanent Secretary, Philadelphia.

MORTUARY STATISTICS.

BY SOLON STARK, M. D., CLERK OF THE BOARD OF HEALTH, ST. LOUIS, MO.

Abstracts of Deaths occuring in the City of St. Louis, for the nine weeks ending March 2nd, 1866.

WEEK ENDING	SEX.	AGES. Under 5 Years.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	Total.
January 5.....	Males.....	16	1	2	1	4	9	6	4	..	..	..	..	43
	Females....	15	7	..	3	2	2	7	4	2	2	..	..	44
" 12.....	Males.....	12	3	..	4	7	5	8	1	2	..	..	..	41
	Females....	11	..	2	2	7	4	2	4	2	..	1	..	36
" 19.....	Males.....	19	1	3	1	2	6	5	3	5	1	..	..	46
	Females....	20	1	..	..	5	5	3	2	1	1	..	..	38
" 26.....	Males.....	24	4	..	..	4	8	7	4	3	2	..	..	56
	Females....	22	3	..	1	6	2	2	2	2	..	..	..	40
February 2.....	Males.....	19	3	..	..	2	6	10	4	..	1	..	..	45
	Females....	21	1	..	..	5	7	2	..	1	1	..	1	39
" 9.....	Males.....	18	..	..	..	5	8	1	4	..	..	..	2	38
	Females....	18	4	..	3	4	2	2	..	3	..	..	..	36
" 16.....	Males.....	17	2	1	..	5	7	2	2	..	2	1	..	39
	Females....	17	1	..	1	8	2	3	2	2	..	..	..	36
" 23.....	Males.....	24	3	4	2	6	5	..	1	1	..	..	..	46
	Females....	19	3	3	1	2	5	..	2	1	1	1	..	38
March 2.....	Males.....	18	..	1	2	7	7	6	7	3	..	..	..	51
	Females....	13	3	2	..	6	6	1	2	3	1	..	..	36
Total.....	.....	323	39	17	21	87	96	67	46	31	12	3	3	748

There were also 73 Infants Still Born.

Statement Showing the Diseases resulting in Deaths, for the Nine Weeks ending March 3, 1866.

Abscess, 7; do of Liver, 3; Accident, 4; Arthritis, 1; Appoplexy, 6; Angina Meligna, 7; Atrophy, 3; Anæmia, 1; Asthma, 2; Aptha, 1; Bronchitis, 15; Burns, 3; Congestive Chills, 8; Convulsions, 58; Consumption, 24; Congestive Fever, 3; Croup, 23; Cholera Infantum, 5; Cramps, 7; Congestion of Brain, 18; Cancer, 4; do. of Liver, 1; Cerebro Spinal Meningitis, 2; Congestion of Lungs, 10; Coma, 1; Cerebritis, 1; Concussion of Brain, 2; Cholera Morbus, 1; Chlorosis, 1; Congestion of Liver, 1; Convulsions Puereral, 1; Diptheria, 16; Debility, 19; Dysentery, 17; Dyspepsia, 1; Diarrhœa, 27; Dropsy, 20; Drowned, 3; Eclampsia, 2; Erysipelas, 7; Endocarditis, 1; Enteritis, 10; Epilepsy, 2; Fever, Pueperal, 13; Fever, Gastric, 1; Fracture of Skull, 1; Fever, Intermittent, 1; Fever, Relapsing, 1; Fever, Typhus, 2; Gastritis, 1; Gun-shot Wound, 2; Gangrene, 2; Disease of Heart, 5; Hydrocephalus, 14; Hepatites, 9; Hydro-thorax, 3; Hemorrhage, 6; Hydronphalus, 3; Hemapthysis, 1; Inflammation of Lungs, 2; Inflammation, 2; Intemperance, 1; Laryngitis, 1; Mania a Potu, 4; Marasmus, 28; Meningitis, 22; Measles, 1; Not stated, 31; Nephritis, 1; Otites, 1; Old age, 4; Phthisis, 69; Paralysis, 7; Pneumonia, 90; Pericarditis, 1; Paralysis of Lungs, 1; Tubercolosis Pulmonalis, 1; Peritonitis, 3; Pertussus, 3; Pyemia, 1; Poison, 1; Rubeola, 1; Rachitis, 1; Rheumatic Fever, 2; Rupture of Womb, 1; Scarlatina, 7; Still born, 73; Softening of Brain, 111; Scrofula, 4; Suicide, 1; Syphilis, 4; Suffocation, 1; Scalded, 1; Stabbed, 1; Typhoid Fever, 32; Tetanus, 6; Tuberculus Abdominalis, 1; Trismus, 6; Unknown, 16; Ulceration of Bowels, 2; Variola, 8.

THE SAINT LOUIS

Medical and Surgical Journal.

MAY AND JUNE, 1866.

Original Communications.

OBSERVATIONS AND SUGGESTIONS ON EPIDEMIC CHOLERA.

BY EDWARD MONTGOMERY, M. D.

[Continued.]

In presenting my views in regard to the treatment of Epidemic Cholera, I feel great delicacy and diffidence. I fear that many may say, I "rush where angels fear to tread." And it is with much trepidation and hesitation that I express my opinion as to the advantages of one course of treatment or the disadvantages of another, and especially, as I believe I differ very materially from nearly all who have written on the subject. I have endeavored to prove and illustrate, by example, the great efficacy of preventive means; and I certainly have great confidence in medicine applied to counteract and conquer the disease in the

system. It is certain that much evil has been done by medical men declaring that all remedies are impotent and useless in cholera; but let the people once become confident that the disease, in a vast majority of cases, can be cured by timely medication, and a great desideratum will be gained. We are told that in a town in the interior of Russia, where the epidemic broke out, two of the surgeons who were sent by the government to attend to the sick died. This created such despondency among the inhabitants that all means were neglected, and the consequence was, that three-fourths of them fell victims.

When the disease broke out in Moscow, in 1831, we are informed that a great panic ensued; both doctors and patients believed that no remedies would avail, and consequently a fearful mortality ensued. Palermo is said to have suffered terribly, in great part from the same cause as Moscow. Palermo lost 25,000 out of her population of 120,000 souls! There is everything, therefore, to be hoped for from timely, judicious, and energetic medical treatment. The medical boards of the Bombay and Madras presidencies report that, of 19,500 cases treated in that birth-place and hot-bed of the disease, 4,430 died; whereas, of 1,300 who got no medical treatment, *every one died!*

When cholera is prevailing, most strict and sedulous attention should be paid to our general health. If nausea, anorexia, biliousness, or diarrhœa is present, a small dose of blue mass or hydrarg. cum cretæ should be taken, and great circumspection and prudence observed in regard to diet and regimen. If the diarrhœa becomes profuse and of the "rice water" character, with vomiting and thirst, it is cholera in its first stage. Perfect rest in the recumbent posture should be enjoined, and a dose of table salt and good mustard administered, from a teaspoonful to a tablespoonful of each, according to circumstances. In thirty or forty minutes after this dose, from two to five grains of the mild chloride should be given every twenty or thirty minutes, until the vomiting and rice water purging is arrested. As it is so exceedingly hard for the patient to swallow medicine or to retain it when swallowed, it will be found advantageous to give

the calomel powders, rubbed up with a little white sugar or applied dry on the tongue, and washed down with a small quantity of cold water, mint, sage, balm, or sweet chamomile tea. Some patients greatly prefer taking their medicine in the form of pill, and there is no doubt but that if a pill is once swallowed, it will remain down better than most other forms. A very small pill can be made, consisting of the twentieth part of a grain of the bichloride of mercury and one grain of extract of hyosciamus. This I have found a very excellent and efficient remedy in the first stage. Another very good and efficacious pill is composed of one-twelfth of a grain of the cyanide of mercury and one grain of extract of quassia. These pills should be made fresh, so that they will easily dissolve in the stomach. The use of one of the mercurial preparations should be persisted in until the vomiting and diarrhœa have abated. A drop of creosote, and four or five grains of sub-nitrate of bismuth, should be administered occasionally during the use of the mercurials. The creosote may be mixed up with gum, sugar, and spearmint water, so as to make a not unpleasant emulsion. I found it very useful in arresting the vomiting, and its admitted antiseptic virtues entitle it to great consideration in the disease under consideration. In this stage, and, indeed, through the whole course of the attack, large injections *per anum* will be found eminently serviceable. They can be thrown up the intestines until they reach the stomach, and probably act doubly advantageous; first, by restraining the exudation of the fluid parts of the blood, and, again, by supplying the tissues and the blood with those ingredients which they so much require. The enemata I have generally been in the habit of employing, is composed of sulphate of quinine, sulphate of iron, sulphuric acid and water; or tannin, sulphuric acid, and water; or a solution of chlorate of potash.

It will be seen that I entirely ignore the administration of opiates and direct astringents by the mouth; and I am aware that in this I am opposed to almost all the medical authorities of the day. I admit that it is all important to arrest the vomiting and diarrhœa as soon as possible; and I contend that this can be done

more promptly and efficiently by the means I have pointed out, than by the popular remedies—sugar of lead and opium; laudanum, camphor, and Cayenne pepper; morphine and tannic acid, or any of the common cholera mixtures heretofore so generally used. In my humble opinion, the results of using opiates and astringents are highly prejudicial: they generally increase the irritability of the stomach, greatly aggravate the thirst, stop the secretions, which we should be anxious to excite to more energetic action; and if they do temporarily stop the diarrhœa, it is, to use a common expression, but "locking the door with the thief in the house."

I have often experienced the bad effects of opiates and direct astringents in cholera. I have seen patients, after taking the prescriptions I have recommended until their diarrhœa and vomiting had abated, and bile reappeared in their stools, who would be persuaded to use some of the nostrums above noticed, when, in a very short time, the vomiting and purging of the copious rice water discharges would recommence. I have in numerous instances had my patients, as I thought, in a fair way of recovery, when some intermeddler or busybody would come in and insist on relieving the sufferer *quickly* by opiates and astringents; and the result of this change of treatment would be, in every instance, disastrous. The action of these medicaments in this disease has been so constantly and uniformly deleterious in my experience, that I cannot conceive how any physician, who has ever treated a great number of cases, can continue to recommend or prescribe them. In ordinary diarrhœa, where we are informed that the patient has copious light-colored, painless stools, do we order astringents and opiates? If we do, we will most assuredly complicate and prolong the case. Nearly all who have written on cholera agree that there is great obtundity of the nervous system, whether by the direct action of the *materies morbi* on the nervous system in the first place, or secondarily from the blood or alimentary canal; still, all are agreed that a certain degree of paralysis or supension of nervous action and energy exists; then why, in the name of all rational medication, should

we administer opiates and astringents to obtain still more this deadened nervous action? There is great reason to fear that many died with comatose symptoms in cholera epidemics, when those comatose symptoms were in great part produced by opiates and the unguarded use of astringents. Opium, in some form, is often given with a view to relieve the sufferer from the muscular spasms or cramps. I think it will be found that opiates will not relieve these cramps, unless it is given in such quantities as will certainly peril the life of the patient. I have always found that the cramps will abate with the amelioration of the patient in other respects; free rubbing of the parts with salt, and mustard and water; or, in the more advanced state of the disease, with a mixture of about ten parts of cool water to one part of aq. ammonia will be found very serviceable. I have never felt convinced of the good effects of hot applications or warm baths in any stage of cholera; it is probable that cold applications and plenty of good cold fresh air is far preferable. As to drinks they should be allowed in moderate quantities, as large draughts will provoke vomiting, tend to burden an already weak stomach, and interfere with the remedial action of the medicine. In the second stage of the disease, when the skin is becoming shrivelled, cold, and of a dark color, it is very fashionable to apply hot applications externally, and give alcoholic stimulants freely internally. It is very doubtful if either is good practice; perhaps very free and constant friction with the hand, and the use of the diluted aqua ammonia before alluded to, well rubbed all over the surface of the body and extremities, with the internal administration of a few drops of this same aqua ammonia in sage, balm, mint, chamomile or cinnamon tea, occasionally will be found the most serviceable. I have many times experienced most happy results from the use of the aq. ammonia in this disease; my friend, Dr. Boisliniere, explains its good effects by saying, that it liquifies the blood, in other words, counteracts that viscid, stagnant condition of the sanguinary tide, and assists its flow from the centre to the superficies. I agree entirely with Dr. Boisliniere that one very important indication, in fact a *sine qua non*, is to

keep the blood circulating, and to effect this we must endeavor to counteract its viscidity, and this, I think, we do by the use of mercurials and ammonia; we must aim to supply it with fluid constituents for those exuded, and this we do by injections of chlorate of potash, chloride of sodium, etc., into the bowels; we should try to impart energy to the nervous system, so that the *vis a tergo* and the *vis a fronti* may assist the free course of the vital current; and this we hope to accomplish by the use of the mineral acids, quinine, etc. I should here mention, that the mineral acids (the nitric and sulphuric) should not be administered by the mouth, at any rate until some time after omitting the mercurials.

The sulphites of soda, potassa, and magnesia, from their known remedial virtues in all cases of septicemia might be worthy of a trial in epidemic cholera. The injection of serous fluids into the veins was practiced by Doctors McIntosh and Latta in Edinburgh, in 1832, and I know they were very successful; but subsequent trials by others are said to have been rather discouraging, still, in desperate cases, this means should certainly be resorted to.

The production of artificial dropsy, by injecting fluids into the cellular tissue, has also been recommended, and, as a *dernier resort*, may be safely employed. The fluid should be inserted into different portions, as the abdomen, the chest, the thighs, etc.

Of the treatment of the third stage, I shall say but little, as there is little difference of opinion in regard to it. If the use of opiates and alcoholic stimulants have not been adhered to, and tonics not extravagantly or injudiciously employed, the stage of reaction will not be hard to manage; a careful attention to diet and regimen, keeping the bowels regular, the head cool, and the extremities warm, perfect cleanliness and good ventilation, will generally be all that will be required. But if severe fever sets in, with typhoid or cerebral symptoms, we must again resort to small and frequently repeated doses of calomel or blue mass, followed by mild aperients, such as Henry's magnesia, Seltzer water, etc. Cupping from the temples and nape of the neck, or leeches around the base of the brain will be necessary. The head

may be shaved, or the hair cut very close and cold lotions freely applied, with occasional cold sponging of the body. If great stupor should supervene, blisters behind the ears, to the back of the neck, and between the shoulders will be necessary. If the stomach is very irritable and the thirst excessive, leeches will have to be applied to the epigastrium, followed by warm poultices; small pieces of ice may be allowed, and the strength supported by mild, bland articles of food and drink.

DISEASES OF THE SKIN—THEIR CLASSIFICATION.

BY PROF. M. L. LINTON, M. D.

The skin is one of the organs of the body, and whatever classification applies to the organs in general, applies to the skin also. The skin is exposed to view, and hence all its changes are objects of sight—we see them, and as their appearances are multiform and various, we have given them as many names as they present varieties in appearance. A simple inflammation of the skin is at first a mere redness, and this has been called *erythema*. But this *redness* may produce *pimples*, and then we call the disease papular. The pimples may become *vesicles* or *pustules*; then the classification is changed, and the disease belongs to the order vesiculæ or pustulæ. Nothing in all the range of pathology is so arbitrary as the classifications of the diseases of the skin: one single disease may belong to all the classes of Willan, Plenck or Bateman. The classifications of these authors are classifications rather of the degrees of disease than of the *kinds* of disease. Indeed, the medical public seems to have come to the conclusion that all the nosologies are failures. Who pays any attention to the classifications of Cullen, Darwin, John Mason Good, and the rest?

Nobody spends time in writing or lecturing against them, either, for they are regarded as already dead.

We repeat here what we have said before, viz: that special pathology is not amenable to classification.

The combinations of elements are too numerous and varied in special diseases to admit of classification. The elements of disease may be classified, and this is as far as classification can at present go; and this applies as well to affections of the skin as those of other organs. Medical writers are, at the present day, pretty well agreed as to the elements of disease and the nomenclature of general pathology; all of them speak very much alike of the hyperemias, anemias, cachemias, hypertrophias, atrophias, etc. They have definite terms for well defined things, and this is the very essence of a natural and true classification. What would be thought of a physician who should classify according to the *wards* of a hospital, *e. g.*, diseases ward No. 1, No. 2, 3, and so on? Yet this would be about as good as camp fever, jail fever, ship fever, mal de Pott, morbus brightii, etc.

But what is the use of poking fun at the old nosologies, after saying that they are dead?

The skin has its alterations of blood and structure, just as other organs, so that this classification has nothing new in it. We have, in former years, insisted on this classification, and we are well pleased that it has been adopted by Hebra, whose arrangement of cutaneous diseases is to be found in an appendix to Wilson's late work on "*cutaneous medicine.*"

To begin, then, there are hyperemias of the skin. Is not this name enough? Is it not as expressive as *erythema*, or exanthema?

These hyperemias are *active or passive*; the *passive* are not so frequent as the *active*. Nevertheless, as examples of the *passive*, may be mentioned cyanosis and the state of the vessels in purpura. Active hyperemia constitutes the majority of skin affections: use the term inflammation, if you will, for inflammation, to say the most for it, but active hyperemia, and some of its results. Say, then, the inflammation of the skin constitutes the

majority of its diseases,—inflammations acute and chronic, *common* and *specific*. The *common* inflammation would embrace all those cases induced by chemical and mechanical irritants. The specific, all those dependent on a diseased condition of the blood, or rather upon a materies morbi in the blood. Under this head alone would come, perhaps, the majority of skin affections; as, for example, the small-pox eruption, that of measles, scarlatina, erysipelas, rheumatic erythema, varicella, and all the scaly diseases, as psoriasis, vulgaris lepra, and the entire range of the syphilides. It might, in many cases, be a matter of doubt whether or not a particular case was a *common* or a *specific* inflammation; but this would be no objection to the classification itself. So much for a bird's eye glance at the hyperemias of the skin. Anemia and atrophy are also diseases of the skin, as of other organs. They are mentioned as such by the authors, though they rarely are prominent elements of disease. There is anemia of the skin in a chill, and in the latter stages of most exhaustive diseases. We cure an anemia of the skin by a hot-bath, though the remedy is generally intended rather for an internal congestion, than a peripheral anemia: whatever changes may take place in any other organ, may take place also in the skin It has its ulcerations, hypertrophias, transformations, deposits, and growths, just as any other part.

Among the hypertrophies of the skin may be mentioned corns, which are but hypertrophy of the cuticle, and elephantias, so called, which is a hypertrophy of the entire skin.

Deposits may occur in the skin as in the lungs, and these may be *euplastic*, *cacoplastic*, or *aplastic*. *Euplastic* deposits merely heal solutions of the continuity, or are organized into a sort of fibrous tissue. The *cacoplastic* and *aplastic* deposits, on account of their deficient vitality, are liable to disintegration and death. The distinction between *deposits* and *growth* is not very clear. The *growths* generally constitute what are called *tumors*. But a mere deposit may be a tumor. Perhaps a growth differs from a deposit in this, that the latter is an exudation, without cell development, and the former is an exudation

with active cell development. However the case may be, the skin is the seat of various *deposits* and *growths*, which differ very much in their nature. The euplastic ones constituting the *benign* deposits, or growths or tumors; and the cacoplastic constituting those which are malign, ordinally called *cancerous*.

Why are they malign? Answer—because they consist of *exudations*, or cells incapable of further organization. They have not life enough to be transformed into a healthy fibrous tissue. Hence they die, they perish, they ulcerate; and the ulcer can never heal, for the *plasma*, or *the cells*, are incapable of organizing the necessary tissues. Of course, there are diseases of the skin produced by external agencies—by chemical and mechanical violence. There are also epizoa, as well as entozoa. Insects may infest the skin, as well as the bowels; but these creatures cause only irritation and inflammation, and the physician gets rid of them on the same principles upon which he would rid his patients of a thorn in the side. He extracts them with forceps. He kills them with mercury or sulphur.

Diseases of the skin, just as diseases of the lungs or any other organ, consist, then, in changes in the quantity and quality of the blood circulating in it, and in changes of *nutrition* consequent thereon; and in external, chemical, and mechanical injuries —that is to say, wounds small and large, with their consequences. In this sketch of the subject, we have not ventured into the nomenclature of skin disease to any great extent. What is gained by knowing that a certain appearance of the skin, or disease of the skin, is or has been called eczema, or psoriasis, or melasma, or ecthyma or acne? Nothing. True, as these names are now in vogue, it is of advantage to know them, otherwise we could not understand what authors were talking about. As there are different languages, we should understand as many of them as possible. But if there were but one human language, it would, doubtless, be all the better for science. We have already spoken of the classification of Hebra, as the most natural and truthful of all the classifications of diseases of the skin; and what does Hebra's classification amount to? Nothing more than an enume-

ration of the hyperemias, cachemias, hypertrophias, cacotrophias, and atrophias of the skin, and the vegetable and animal parasites which may infest its surface. We say nothing more than these; but we must except certain functional aberrations of the skin mentioned by Hebra as diseases, but which are nothing more than *symptoms* of disease.

The following is Hebra's division: 1, hyperemias; 2, anemias; 3, altered secretions of the skin; 4, exudations; 5, hemorrhagics; 6, hypertrophy; 7, atrophias; 8, *neoplasms*, or new formations, of the homeoplastic kind; 9, new formations of the heteroplastic kind; 10, ulcerations; 11, nervous diseases of the skin; 12, parasites.

He distinguishes hyperemia into active and passive; and, again, into *common* and *specific*, or, in his language, into idiopathic and symptomatic.

Under the head of active idiopathic hyperemia, he places erythema from wounds, from heat, and from irritants in general. Under the head of symptomatic hyperemia, he ranges certain *rashes*, as roseola, or, as he calls them, eyrythemas, dependent on some humor in the blood. As examples of *passive* hyperemia, are mentioned cyanosis and some other mechanical impediments to the circulation. Anemias of the skin are mostly secondary, and result from losses of blood and weakened circulation from any disease.

Under the head of changed secretions, he enumerates increase and diminution of the sebaceous secretion and the *sudatory*. Thus he mentions hyperidrosis, or too much *sweating*, and anidrosis, or diminished sweating; also menidrosis and galactidrosis, or sweating of the menses and milk.

What he calls the *exudations* are the various *specific* inflammations of the skin, and there is in them an *exudation*, which there is not in mere hyperemia. They are *variola*, *vaccina*, and various papular affections, erysipelas,—boils, carbuncles, etc. We shall not enter into his details of this division; suffice it to say, that this division includes a vast portion, even the main por-

tion, of skin diseases, as the lepra vulgaris psoriasis, the syphilides, eczemas, impitigo, etc., etc. Under the head of hemorrhagia are placed the purpuras, idiopathic and symptomatic. Amongst the hypertrophias are mentioned *corns*, which are hypertrophias of the cuticle, and warts, which are hypertrophias of the papilla, and eliphantiasis, which is a hypertrophy of the whole skin. We mention these merely as examples of the sixth division of Hebra. Overgrowths of nails and hair of course come under the same head; so also the increase of the coloring matter, as lentigo and *melasma*. According to Hebra, the *black* skin is a sort of hypertrophy, whereas the *white* skin is an atrophy. The negro is *plus*, and the white man *minus*, in this respect. Of *atrophia* of the skin, there are five or six varieties mentioned, but the name sufficiently signifies their nature. Of the *neoplasms*, the non-malignant growths and formations constitute one division, and the malignant—namely, cancer and tubercle—the other. The *ulcerations* are distinguished into idiopathic and symptomatic. The *neuroses* are the hyperesthesias and anesthesias, local and general. The *parasites* are the dermatophyta and the dermatozoa. The so-called cryptogamic plant, said to be found in Favus, represents the former, and the common pediculi the latter.

Such is an outline of Hebra's arrangement—the best of its kind. Nevertheless, we object to it in some respects; for example, he has confounded *structure* and *function*. Most of his classes consist in derangements of structure. But two of them are merely *functional* aberrations, as, for example, his third class—his alterations of *secretions*. *Secretion* is not a structure, it is a function; and it must be plain to every one that too much or too little perspiration, or alterations in the quality of the sweat, are *symptoms* of disease, and not diseases themselves: whatever change in the skin itself, or in the general system, causes these altered sweats is the disease. When the secretion, as of the sebaceous follicles, is retained, then little tumors, or *secondary* diseases, may result; so when blood is effused into the tissues of organs, the result is a secondary organic change or

disease. So is dropsy a secondary disease, the result of the effusion of serum.

The same objection must be made to the *neuroses* of the skin; itching, excessive sensibility and loss of sensibility are clearly but symptoms of disease. We need not insist on this. These two divisions excluded, the classification of Hebra amounts only to the announcement of this formula, viz: diseases of the skin consist in alterations of the quantity and the quality of the blood of that organ, and in certain changes in its nutrition, as hypertrophy, atrophy, cacotrophy, etc., and in certain injuries caused by external chemical and mechanical violence. In other words, the classification of diseases of the skin is just that of other organs, and nothing more and nothing less. Such we conceive to be the classification of nature and common sense.

To understand the pathology of skin diseases, something more is required than committing to memory a great many hard names, or in knowing that such an eruption is called *erythema*, and such another *roseola;* that this is eczema, and that impetigo. We do not say that it is not important to understand these terms, now that they are in vogue: we must, as before remarked, understand them to comprehend what authors are writing about. It is well to understand them just as it is well to be acquainted with different languages, so as to benefitted by the authors who write in those languages. At the same time it would doubtless be better for science had there never been but one language. The confusion of tongues was a barrier to science: it hindered the building of a great tower some thousands of years ago; and the confusion of tongues which yet remains has been an obstacle in the path of medicine and of science in general. It was an acute philosopher who said that science is a well made language. "La science est une langue bien faite." But before we can have such a language as this, we must know the nature of things and processes. Then we can name them, and simply to name them aright, is to classify them.

IMMEDIATE, PRIMARY AND SECONDARY AMPUTATIONS, AND EXSECTIONS OF THE FEMUR FOR GUN-SHOT INJURIES.

BY E. A. CLARK, M. D., ST. LOUIS, MO.

The opportunities offered to medical men, during the war of the past five years, have been apparently ample to decide many mooted questions in surgery, which, prior to that time, the profession in America did not have an opportunity to determine. Yet it would seem that, with all this vast field for observation, there are still those—whose opinions are entitled to great consideration—who maintain views at variance with the practice of immediate amputations and exsections in gun-shot wounds of the femur, amongst whom is Prof. Frank H. Hamilton, of New York, who, in his recent work on military surgery, condemns the practice of immediate amputations, but advocates the primary operation, which condition he allows to commence after reaction is established, and continue until inflammatory symptoms ensue, which is, perhaps, the most correct definition of this period that we can adopt. But Dr. Hamilton thinks that the experience of the profession will not justify immediate amputations, as a rule; that is, in the period intervening between the time the wound is received, until reaction is established, embracing, ordinarily, a period of twelve hours. In addition to other evidence in favor of primary and against immediate amputations, Dr. Hamilton alludes to an article of ours, published in the *Chicago Medical Journal* for June, 1863, in which we reported nine cases of immediate amputation of the leg, and five of the femur, all of whom recovered but one case of amputation at the thigh. Nine of these cases occurred at the battle of Pea Ridge, in March, 1862, and we stated explicitly in our report that "we amputated at once on the field," meaning, of course, before reaction was established, or, in some cases, before the system had fully yielded to the "shock." These were instances in which that condition

was delayed, that is, where the men, excited from the heat of battle, did not succumb at once to the depressing influences of the shock; though Dr. Hamilton says that he has never met with such instances in his experience, "except where some visceral lesion or the rupture of a large blood-vessel has accompanied the accident." But amongst medical men of the army we think there can be adduced much evidence in proof of this statement, that the nervous concussion may be delayed even for hours after the receipt of the injury. McLead says that he knows of several well authenticated cases in evidence of this fact. How often do we meet with men upon the field, who, when receiving severe flesh wounds, are perhaps unconscious of it until they discover their boots filling with blood; or even when there has been but little hemorrhage, and after a considerable delay the patient has been seized by a severe nervous commotion and excessive depression. However, we do not contend that this delay of the shock occurs in every case, nor do we offer it as a rule by which we should be governed in deciding to make immediate amputations. But we contend that, as a rule, amputations should be made before reaction is fully established, whether that be during the period of delayed concussion or not. But in Dr. Hamilton's notice of the above cases, he seems to imply that we must have amputated at the period he designates as "primary," because we subsequently called these amputations primary, but not in the sense in which he means that term. In that report we did not make any distinctions between immediate and primary operations, as all the cases that were not operated upon immediately were delayed until suppuration had been established; and having used the term "immediate" in the first instance, was sufficient to qualify at what period of the primary stage we operated.

In the same report, referred to above, we subsequently mentioned two cases of successful amputation of the thigh, and three of the leg, occurring at the battle of Prairie Grove, in which it was positively stated that the operations were performed "immediately, before reaction was established." Yet Dr. Hamilton says, that "we would have been better satisfied if we had been

informed about how many hours had elapsed in each case." Apparently with a view to distort our meaning into a delay, and thus corroborate his position, when it could not matter how many hours had elapsed; if, as stated, the amputations were performed before reaction was established,—we certainly could not have been understood to mean the period he designates as "primary." Yet, with this exception, Dr. Hamilton discusses the subject very fairly, and we admit that his opinions are entitled to much credence. However, we think the majority of the profession will not sustain his views upon primary amputations to the exclusion of the immediate, unless it should be in peculiar cases where there is excessive depression and collapse, consequent upon a badly lacerated wound with great loss of blood, and the patient is perhaps moribund. Of course, no rational man would think of amputating at such a crisis; and, indeed, we have never seen any such cases recover, when amputated after the period of the greatest reaction had been established. When, from such depletion, all the vital powers of life are flagging, and nature is struggling to economise the very blood that is ebbing the life away, it would certainly be most hazardous to tax the constitution with a greater loss of blood; or the nervous concussion consequent upon amputating at such a period, or even until after the system had recuperated from its excessive loss of blood, which would likely, in most cases, delay the operation until the period designated as "secondary." But, we admit, that there are cases, where a considerable hemorrhage has been sustained, with a great degree of concussion, in which it would be advisable to delay amputating until reaction is fully established. Though we hold it to be true as a rule, that where the patient has not suffered depletion from hemorrhage, he will react almost as readily from an amputation of a lower extremity performed immediately after the receipt of the injury, as he will from the injury alone. Where a patient can retain all the blood in his body that was required to sustain him in health, he can resist a single shock to his nervous system of much greater force, than he can repeated blows of less force, at intervals sufficient to allow more or less complete reaction, by

which means his ability to react becomes exhausted, and each time his efforts to recover are more feeble. We must all admit that a man is in better condition to endure an amputation before, than after he has received a wound sufficiently severe to require such an operation; because, we say, that then he has all the vigor and stamina of health to sustain him and insure his more ready and prompt reaction; that he is not laboring under any exciting influence, the result of previous depressing causes which have depleted his nervous energy. Then, if this condition is the most favorable for operating, why is it not the next best moment to follow the wound with the knife at once, and, as it were, allow the surgeon's wound to supersede that of the missile, while we still have all the latent vigor of health to come to the rescue and secure a permanent reaction, before the constitution has become exhausted by repeating such efforts. This principle can be no better illustrated than to observe the apparent impunity with which a soldier receives a second wound while lying upon the field prostrated from the shock of a previous one; he will react apparently as quick from both wounds as he would have done from the first alone. This is also equally true in amputating the lower extremities immediately after the wound is received, while the condition of shock still exists, when the patient, with a single effort at reaction, will survive both the wound and the operation with but little more delay than will be required in establishing reaction from the wound alone; provided, as we have before observed, that the patient is not depleted from excessive primary hemorrhage. However, it matters but little whether we can explain the rationale of this treatment or not; the statistics, and experience of the most observing men in the profession should be the proper criterion to decide our action in these cases.

Amongst those whose views corroborate this plan of treatment, we find the names of Ambrose Paré and Wiseman, the latter of whom was surgeon to Charles the II., who both affirm that amputations should be performed immediately. Larrey states, also, that "I have lost a greater number of soldiers, because, although operated upon within the first twenty-four hours; yet

the operations had been made too late." Duboy, in speaking of the war of the Revolution in America, says, that "American surgeons amputed at once, and lost but few; but that the French delayed and lost many." Hutcheson and McLeod also confirm the statements of these distinguished observers. Dr. Lidell, Surgeon, U. S. V., late in charge of the Stanton General Hospital at Washington, also favors immediate amputations, and thinks that the shock is often delayed. Dr. J. H. Thompson, U.S.A., reports, in the *American Medical Times* for July, 1862, twelve cases of immediate amputations, with but one fatal result up to the thirteenth day, when he last saw them. The fourteen cases of immediate amputations of the lower extremities referred to above, and which occurred in our own experience, with but one fatal result, is also additional evidence, showing a lower rate of mortality than we have ever witnessed from the primary operation in a similar class of cases. And when compared with secondary amputations, there can be no data instituted by which to justify the latter operation, except in cases which indicate a favorable prognosis in the beginning, with a prospect of saving the limb without an operation; when, subsequently, symptoms occur precluding the possibility of so favorable a result, then, of course, there is no other remedy but secondary amputation. But, in cases where it is probable that amputation will be required, we hold, that, as a rule, it should be done immediately; or, if from any contingency, it is not deemed practicable to operate thus early, the period immediately following reaction should be our next choice in preference to waiting for the secondary operation, which, beyond a doubt, is the least desirable period at which we can amputate. In addition to the immediate amputations referred to in the above report, we reported also twenty-eight cases of secondary amputation of the lower extremities, performed after suppuration was established, all of whom died but four, notwithstanding these wounds were received in the same engagements that the others were, who were operated upon immediately; though eighteen of these cases occurred at the battle of Prairie Grove, at a time when the army was almost exhausted from long

continued night marches, so that the wounded were in the most unfavorable condition to recover, and soon sank from repeated attacks of hemorrhage and excessive suppuration. The great prevalence of secondary hemorrhage amongst these wounded necessitated many amputations, that, under other less pressing circumstances, should not have been performed. However, at best, the rates of mortality would have far exceeded both the immediate and primary operations, as we have tested to our satisfaction from subsequent experience. But from these observations we would not have it understood that we are over anxious to amputate every case of gunshot fracture that comes under the surgeon's care: by no means. In fact, we are opposed to amputating, as a rule, in comminuted fractures, whether of the upper or lower extremities, except in cases where too large a segment of bone has been carried away; or where the shaft of the bone is split for a considerable distance, involving, perhaps, the articulation of the knee joint; or where a large artery has been severed; in such cases amputation seems imperative. But such extreme injuries constitute a small portion of all those who fall from gunshot fractures during an engagement; and we are of the opinion, that if more discrimination had been exercised in such cases on the part of many surgeons during the late war, that there would have been a less number of amputations performed, and, perhaps, more lives saved. The means we refer to, in such cases, is to enlarge the wound by incision—where it is necessary—and remove all the loose fragments of bone, together with the pointed spicula which are denuded of periosteum; or where both the fragments are very oblique, and, as sometimes happens, the pointed extremities come in opposition upon the same line, leaving a large excavation of bone to be supplied; these projecting fragments should also be removed, even if they are found to be covered by periosteum, and, by this means, bring a larger surface of the bones into opposition, which will insure a more prompt and permanent union. In performing these exsections, we seldom require any other instruments than the bone forceps, or, perhaps, occasionally, the chain saw, where

the fragments are very large; but we think it is seldom, if ever, necessary to turn the ends of the bones out of the wound to remove their pointed extremities. This operation Dr. Hamilton thinks also of doubtful propriety when performed upon the lower extremities, especially in fractures of the femur; and in his notice of an article of ours, published in the *Chicago Medical Journal* for June, 1863, in which we reported four successful cases of exsection of the femur, he says: "Indeed, we must say, candidly, that we have never seen a case which would justify the operation." And in offering his only reasons for disapproving the operation, he says: "The great depth of the bone, and the attachment of the muscles to almost every point of the surface of the bone, are perhaps the chief circumstances which render these operations unsuccessful." These reasons we cannot regard as sufficient to invalidate the success of this operation, for there is no greater proportion of the femur occupied by the attachment of muscles than there is of the humerus, where Dr. Hamilton is in favor of exsections. And, as regards the great depth of the femur, we cannot understand why that should deter us from operating, when it will probably not require an incison of more than three inches in length; and when made through the wound, a part of the contused tissue through which it passes would slough away, even without such incision. Moreover, we believe that, in a large number of these cases, where there is sufficient of the continuity of the bone destroyed to render this operation necessary, there is but one other remedy left, and that is amputation, which we think would inflict an incision of much more gravity than the simple wound required, to remove the fragments from the fracture, even were other things equal. Such reasoning does not seem consistent, when Dr. Hamilton, in his same work, approves of exsections for gun-shot injuries of the neck of the femur and trochanters, in preference to amputating, at which point there is more surface occupied by the attachment of muscles than at any other on the shaft of the bone; besides, the incision required would be equally as deep and extensive as at any other point. In speaking of exsections of the

shaft of the femur, Dr. Hamilton says that "certain loose fragments" may be removed; when, if these fragments are larger than the missile that inflicted the wound, as is invariably the case, it will certainly be necessary to make an incision into the fracture for their removal; and while the fracture is thus exposed, it would seem to be the most rational treatment to remove all the projecting points of bone that are denuded of periosteum, and which must act, as foreign bodies, to delay union in the fracture, until they are removed by necrosis, and thus exhaust the constitution of the patient, in performing a work which a moment's interference on the part of the surgeon would have accomplished; and, at the same time, shape the ends of the bones, so that they can be brought into more perfect apposition, which would otherwise be prevented by these projecting points placing the ends of the bones at too great a distance from each other for the space to be filled up by osseous deposit, in the same manner that a ball or any other foreign substance would do.' It should be as much a rule in surgery to remove all fragments and denuded projections of bone, in a gun-shot fracture of the femur, as to remove balls, pieces of clothing, or any other foreign bodies lodged in the fracture. In the report referred to above, we mentioned four cases of exsection of the femur, for gun-shot fractures, all of which were successful. Since that time we have performed the operation ten times, with but three failures, and they were cases in which the operation was delayed some weeks, while all those who were operated upon soon after the injury was received, recovered without any unusual delay. These cases occurred in our experience whilst in charge of the general hospitals at Brownsville and Little Rock, Ark., during a period of two years. In addition to these cases, we treated also five cases of fractured femur, by only removing the loose fragments, while several projecting spicula of bone were allowed to remain untouched, some of which were denuded of periosteum. Three of these cases recovered, and two died in from four to six weeks, with apparently but little effort at union in the fracture; while those who recovered were much more tardy in establishing union than those upon whom

exsection had been performed. With these results occurring under our own observation, and considering the great fatality attending amputations at the thigh, as well as that of non-interference in comminuted gun-shot fractures of the femur, we cannot refrain from insisting upon exsections as possessing advantages superior to all other means in the treatment of gun-shot fractures of the femur, as well as of the upper extremities.

ON COLOR-BLINDNESS, OR DALTONISM.

BY MONTROSE A. PALLEN, M. D.

Under the general term of Daltonism, as bestowed by Prevost, Wartmann, and others, upon color-blindness, we propose to consider that abnormity of vision, which has not only excited the curiosity of the physician, but which has also occupied the earnest attention of poet, philosopher, and metaphysician. Dalton, Seebeck, Szokalski, Purkinge, Goethe, Brewster, Cunier, Combe, Dugald Stewart, Herschell, Ruete, Graefe, Hays, and numerous others, have theorized on its nature and its causes, but have been most unsatisfactory in their results.

In 1858* we advanced a theory to explain the phenomena in question, which subsequent thought strengthens. Before entering into that portion relevant to its explanation, it is proposed to consider, as cursorily as possible, its nature and history.

There are two kinds of Daltonism, viz: *Achromatopsy*, or an insensibility of the eye to colors, and *Dyschromatopsia*, an anæsthesia, or partial insensibility to colors. One is generally, if not always, hereditary, and the other is sometimes acquired,

* "PRIZE ESSAY" *on Vision, and some of its anomalies, as revealed by the Ophthalmoscope.*

and subjectively symptomatic (spurious, and recognized by the ophthalmoscope).

The first is dichromatic, and persons who labor under such conditions, easily distinguish the forms of objects and the gradations of light and shade, who not only are capable of seeing objects at great distances, but read with ease where the normal eye cannot. This class of Daltonists are absolutely color-blind, and are dead to all beauties of color, and necessarily ignorant of those branches of knowledge dependent upon colors. The second, which is also hereditary, sometimes acquired and subjectively symptomatic, is polychromatic, and presents a great variety of shades, from simple retinal congestions to such anæsthesia as border upon the dichromatics.

The influences of race seem to have been noticed, and Szokalski states that it is of Germanic origin, being particularly observed among the Germans, English, Swiss and Belgians,—the French, Spaniards and Italians being comparatively free from it,—an immunity which we think to be altogether inexplicable, so much so as to occasion great doubt as to the correctness of the statement. In the United States, numerous instances have been observed, and no mention is made that the Latin race, or their descendents in this country, are freer from the ills that flesh is heir, to than are their brethren, whose ancestry came from the vine-clad hills of the Rhine. Sex undoubtedly has some influence, perhaps a very great one, for, according to Dr. Pliny Earle, in thirty-one cases, twenty-seven were males. Wilson, White Cooper, and others, have recognized the same proportions. Cunier,* however, mentions a fact that *Daltonism* occurred in five generations of one family, making thirteen cases, all of whom were females. White Cooper states, however, that these cases stand alone. With regard to any physical phenomena presenting themselves in *Daltonists*, there is great variance among writers, Szokalski, Ruete, Himly, and White Cooper denying such; whilst Rau and Wartmann draw attention to a yellowish or golden tinge of the iris, which possibly and probably is nothing more

* *Annales d'Occulistique*, tome 1, p. 417.

than a coincidence, as *Daltonism* has been observed in as many blue, grey, and black irides as in the hazel or golden-tinted. The great importance of this subject is readily recognized when we take into consideration the numerous signals used upon all routes of transportation and travel, and the liability to errors, such as to occasion great loss of life, from a misapprehension of railway and steamship colors. The attention of our legislative assemblies should be called to the fact that a rigid examination with regard to colors should be instituted in the cases of steamboat pilots, railway engineers, and guards, particularly when we remember that nearly five per cent of the human race are more or less color-blind.

Dr. Wilson states, that out of 1,154 persons examined in the city of Edinburgh alone,

1 in 55	confounded	red	with	green.
1 in 60	"	brown	"	"
1 in 46	"	blue	"	"

The proportion, with respect to blue and green, was an accidental discovery; but, according to Dr. Wilson, with regard to red and green, and brown and green, they are but "degrees of the same affection; all in the first category, red with green, must be added to those in the second, brown with green; and, many of those in the second might appear in the first; but no one was counted more than once." Numerous other facts can be induced to sustain this point; but, for the present, we proceed to the consideration of the other bearings of the subject.

The *polychromatic Daltonists*, those laboring under dyschromatopsia proper, are subdivided into two classes. A. Those who differ in the degree of their vision, yet agree very nearly in confounding the following colors (according to Seebeck):

1. Sky-blue, grey-blue and grey-lilac.
2. Lilac and bluish-grey.
3. Bluish-green and imperfect violet.
4. Crimson, dark green and hair-brown.
5. Rose-red, green (rather blue than yellow) and grey.
6. Pure light green, grey-brown and flesh color.

7. Intense orange, light yellowish green, or brownish green, and yellow-brown.
8. Light orange and pure yellow.

Daltonists of this class are in reality in a transition state from dichromatism to polychromatism, and have generally a very imperfect idea of all colors; but, particularly, confound red with green, which is dull grey; blue also seems grey to them. Some distinguish yellow tolerably well. B. Those of the second class have only an imperfect perception of the least refrangible rays, and recognize yellow best, distinguish red better, and blue not so well as the others. The principal confusion arises with—

1. Dark violet and dark blue.
2. Crimson and violet.
3. Rose-red, lilac, sky-blue and grey (including lilac.)
4. Brick-red, rust-brown and dark olive green.
5. Cinnabar-red, and dark brown.
6. Imperfect (somewhat yellowish) rose-red and pure grey.
7. Flesh-red, grey-brown and bluish green.
8. Dark carmine and blackish blue-green.
9. Light orange, greenish yellow, brownish yellow and pure yellow.
10. Bright orange, yellow-brown and grass green.

These are the classes which are principally met with, and the question now arises, is there no fact we can grapple to elucidate the cause, and possibly the remedy, for Daltonism? We believe there is; at least, analogy leads us so to do.

In a short paper like this, we cannot enter into the discussion of the various optical laws upon which vision is dependent; but it is well to remember, that but a small portion of the rays of light which are impinged upon the eye enter its cavity, and not all of those which enter, reach the retina; a certain quantity, of necessity, being lost from the numerous refractions undergone in passing through the aqueous humor, lens, and vitreous body; in fact, it is only the more central rays which pass through the pupil and lens. The structure of the lens necessitates an increase of the convergence of the rays entering and passing out of it; and those which reach the vitreous body do so in a highly concentrated stratification. Upon the formation of the vitreous body, we believe the explanation of the theory of color-blindness depends.

According to many distinguished authorities, the vitreous body is surrounded by an *enveloppe*, called the hyaloid. Robin, however, denies this, and contends that it is an amorphous, not even fibroid substance, surrounded posteriorly and laterally by the retina, and anteriorly by the capsule of the chrystalline lens and ciliary processes. Hannover, however, and many others, contend that the vitreous humor is contained in a segmentary membrane, which can be discovered (according to Hannover, particularly) "by a careful maceration in chromic acid, to consist of about one hundred and eighty delicate septa, like the pulp of an orange," with the angles of the inclosed spaces in the direction of the axis of the eyeball, which do not meet, but leave a cylindrical portion for the passage of the *arteria centralis retinæ*. Bowman has confirmed Hannover's experiments in the fœtus. Kolliker thinks that it is an analogous condition of the embryonic areolar tissue, which, after fœtal life, disappears, and becomes a kind of more or less consistent mucus.

Most physiologists agree that the functions of the vitreous consist in the correction of prismatic refraction. Its relations to Daltonism will be explained farther on.

Numerous explanations have been offered to account for the phenomena of color-blindness. We cannot entertain all of them, but propose to cursorily glance at the theories of some of the most eminent and intellectual men who have attempted to solve the problem. Dalton, the author of the atomic theory, Dugald Stewart, the metaphysician, Mr. Troughton, the celebrated optician, Prof. Brandis, Dr. Somers, Dr. Unzer, and many other eminent men, have been the victims of the infirmity; and, of course, have strenuously striven to account for it, and, *par consequence*, to devise the proper remedies. Dalton's theory was, that one of the humors of the eye was a "colored medium, probably some modification of blue." He further remarks, "I suppose it must be the *vitreous humor*; otherwise, I apprehend it might be discovered by inspection, which has not been done." A post mortem examination by Dr. Ransome failed to discover any discoloration of Dalton's vitreous humor. Notwithstanding this

fact, Trinchinetti, an Italian physician of some eminence, has revived this hypothesis, and states his belief to be, that Daltonism is produced by a coloration of one or more of the diaphanous media, probably the chrystalline, and even advises its extirpation to remedy the defect. Goethe explained it by an insensibility of the retina, or sensorium, to blue color; and, on the contrary, Szokalski, in enumerating more than sixty patients, failed to detect one in which there was absolute deficiency of the perception of blue.

In such cases, many have imagined that the retina has a blueish tinge; the ophthalmoscope now, however, places all such theories out of the question, because a blueish tinge of the retina would indicate not only dyschromatopsia, but a condition of a much more formidable nature. Dr. Thos. Young explained it on the theory of a "paralysis of those fibres of the retina which are calculated to perceive red,"—nothing more than a *quid pro quo*. Dugald Stewart explains it by a want of the adaptability of memory to color, or an incapacity to "conceive the sensative distinctly when the object is removed,"—a *post hoc propter hoc*.

Many learned men metaphysically inclined, for the want of a better one, have adopted Stewart's theory. Phrenologists attempt to account for it by the absence of the organ of color; this, too, is a mere nothing by the way of explanation. One of the best rationalistic explanations is that cited by White Cooper as having been advanced by Hartmann; and did we not possess physical explanations, would be that adopted as the most satisfactory. "Hartmann is of opinion that it is by analysis that we arrive at a knowledge of objects which present themselves to our notice; he supposes that we do not perceive them instantly, but little by little, and only by examination of their distance, form, and color, which scrutiny rests on a series of changes operating on the retina, ciliary nerves, and motor ocular nerves; we do not easily recognize objects unless this succession of modification has become habitual, and takes place easily, hence achromatopsy results from a certain state of torpor and indolence of the retina and motor muscles of the eye!"

In his *Second Memoir on Acromatopsia*, Wartmann says: "I admit, with Harvey, Young, Jungken, Muller and others, that its seat is in the retina, and I think that it is produced by an abnormal state of the nervous expansion, in such sort that it reacts equally under two or more differently colored vibrations. If the vibration caused by a ray of red is identical with that engendered by a green ray, there will be confusion of these colors. This theory is independent of all systems destined to explain light. The theory which explains *Daltonism* by an abnormal elasticity of the retina, has the advantage of substituting a reasonable physical condition for a vague notion of the sensorium; besides, it is supported by facts, because the injuries which alter the ordinary constitution of the visual organ are capable of exciting permantly or temporarily a false perception of colors. Lastly, it appears to be confirmed by the circumstance, that, with many *Daltonians*, the eye sees less distinctly the red rays than those which the refrangibility is greater." Hannover's discovery of the segmentary disposition of that body, after its maceration in chronic acid, seems to have been neglected by all writers on Daltonism. We feel confident that an explanation of the phenomena of color-blindness can be made by such segmentary and prismatic arrangement. The explanation we offer, and which we propose to prove by an analysis of prismatic refraction, is, that *color-blindness is the result of irregular prismatic refraction, producing interference of rays of light as it passes through the vitreous humor.* What conditions combine to produce such an effect, physical research has, as yet, been unable to demonstrate; but *a priori* reasoning by exclusion tends to that effect. For there are bodies which will transmit one kind of color and reflect another—leaf gold for instance, which transmits yellow, but reflects green. Again, some substances only reflect the various rays, thereby producing color when they are of a certain thickness, beyond which they appear black. Now for the proofs. The peculiar property which a triangular prism of glass possesses of refracting light, and decomposing it into seven primary colors, viz., violet, indigo, blue, green, yellow, orange, and red, is the basis of our demonstration.

The solar spectrum with regard to each particular color is vivid only at a particular part, as their blending is of such a nature as to make it difficult to say where one ends and the other begins. The violet rays bend most from their course; the red the least, or are the most direct; the indigo, blue, green, yellow, and orange possess this property as they are named in this list. These colors were supposed to be homogeneous, or simple, because they preserved their individuality when passed through another lens. Some of these colors are compound, as we can synthetically demonstrate. Red, blue, and yellow have been used by artists, since time immemorial, in the formation of all other tints; and as no other mixture would produce these colors, they must be necessarily simple, or homogeneous, and the others, viz: violet, indigo, green and orange, as a natural consequence, are compound, or heterogeneous. Buffon first noticed that, by steadfastly looking at a red, yellow, or blue spot on a black or white ground, a fringed border is seen around each of them, composed of the three tints, viz: around the red spot a green border—and green is composed of yellow and blue; a violet border is observed around the yellow spot—and violet is made up of blue and red; orange border is perceived around the blue spot—and orange is a mixture of red and yellow. These phenomena remained unexplained until Mr. Hay, of Edinburgh, brought them before the public, in his work on the *Laws of Harmonious Coloring*. Sir David Brewster, Dr. Neil Arnot, Prof. Holmes, and many other scientific persons, have concurred in the opinions advanced by Mr. Hay. In fact, his experiments can be easily verified, as they have been done, by any one who chooses so to do, by fixing a prism in a hole in a shutter, admitting a ray of light in a darkened room, and the decomposed light thrown upon a screen. Each color is then put to the test, and is found incapable of being divided into two. A hole is then made in the screen, corresponding to the centre of the red of the spectrum, and another in that of the blue, and these colors allowed to fall on a second screen. By means of a second prism the red ray is directed to the spot where is the blue, and the result was a combination of the two,

and a pure and intense violet produced, exactly similar in all respects as the violet on the first screen. Red and yellow subjected to the same process develops orange. Yellow and blue thus treated, produces the prismatic green. A simple ray, red for instance, was then thrown upon a compound one, green, and there was no union; as, on the contrary, there was an interference, and a negative result obtained,—black. Thus it was with the whole series. Thus, by synthesis, it is shown that the three homogeneous colors yellow, red, and blue, have an affinity for each other, which is wanting in violet, indigo, green, and orange; and, as a consequence, could not be the same in every respect save color and refrangibility. The three homogeneous colors yellow, red, blue, are in a numerical ratio as follows: yellow, three; red, five; blue, eight; and when any opaque body reflects these colors in such proportions, white is the result. In this condition they are in an active state, and each is neutralized by the relative effect of the others. When in the passive state, these rays are absorbed, and black is the result. The effect is the same when transmitted through a transparent substance; but are *material* or *inherent* in the first case, and *impalpable* or *transient* in the second. Hence color is not an entity, but depends upon the refractive or reflective power of bodies; and, as the disposition of the atoms or particles of various bodies, so is their power of reflecting and absorbing rays; and as the reflection and absorption, so is the color, which is not inherent. Color produced by artificial light, viz: oil, candles, gas, etc., the rays of which are not so pure as those of the sun, is very apt to be deceptive, so much so that every one will mistake pale blue for green, or *vice versa*. Now, as color is the reflection of the rays which the body does not absorb, an interference of such rays in passing through the vitreous produces the phenomena of Daltonism, when there is a defect of prismatic arrangement in that body. A rose is red because it absorbs all the rays of the prism, except red, which it reflects; but look at it through the medium of a compound color, green for instance, and an interference takes place, and black results.

Each prismatic ray has a different momentum, and it requires different distances to bring them to a focus. The red having the greatest momentum, is soonest brought to a focus; therefore the sun looks red in a fog. When we remember that light has a regular recurrence of periodical movements, at equal intervals, (500,000,000,000,000) five hundred millions of millions in a second; and that if one vibration alone were lost the color would be imperfect, we can form some conception of the minutest difficulty in the way of vision, infinitely too small to be appreciated by anything in the nature of physical research.

Sir John Herschell seems to have arrived at the most correct data with regard to the length and rapidity of the various rays of the solar spectrum. We quote them as follows:

Colored rays.	Length of luminous rays in parts of an inch.	No. of undulations in an inch.	No. of undulations in a second.
Red	.0,000,256	39,180	477 billions.
Orange	.0,000,240	41,610	506 "
Yellow	.0,000,227	44,000	535 "
Green	.0,000,211	47,460	577 "
Blue	.0,000,196	51,110	622 "
Indigo	.0,000,185	54,070	658 "
Violet	.0,000,175	57,490	699 "

According to the calculations of Frauenhofer, the relative intensity of light of the various portions of the solar spectrum are numerically expressed as follows:

Violet	from 5 to 6
Blue, dark (indigo)	81
Blue (of solar spectrum)	170
Green	480
Between Orange and Yellow	1,000
Orange	640
Red, middle ray	94
Red, extreme ray	22

These tables are introduced simply to demonstrate that the different colored rays of the solar spectrum may be accounted for by a difference in the frequency of recurrence in the vibrations. That light is transmitted by undulations, in a vibratory manner, we will not discuss, as such is believed by all scientific observers.

The deductions we reach, after a careful consideration of this subject, are, that if Daltonism be not dependent upon a disarrangement of the prismatic segmentation of the vitreous humor, such an hypothesis readly explains all its phenomena. We feel satisfied that a polychromatic Daltonist, such as Dr. Darwin, the poet and botanist, who could distinguish a cherry from the leaf only by its form, both being gray in color, labored under such deformity in regard to prismatic refraction as is above mentioned, when speaking of the interference of a simple and compound ray. The dichromatic Daltonist, or one who distinguishes only black and white, evidently lacks the proper prismatic refraction, because there is a too rapid concentration of rays, which also goes to explain the facility with which they read in darkness.

With regard to such cases of temporary Daltonism as are frequently encountered in practice, the explanation is readily understood as being subjective, and dependent upon a pathological condition of the visionary apparatus. We have seen an instance of this, in which the ophthalmoscope revealed *hyperæmia retinæ et papillæ*, and which disappeared under the proper treatment, as did the disease. As for the remedies, which are entirely of a physical nature, a complete study of the character of the phenomena will necessarily induce the use of such glasses, tinted to throw such rays into the cavity of the globe as will in a measure make up the deficiency caused by defective prismatic derangement of the vitreous body. These, of course, must be determined by a synthetical study of such colors as are non-appreciable to the Daltonists.

Jungken and Chelius recommended the use of colored bands with their names attached, in order to facilitate the defective memories of the Daltonists, based on the theory of Stewart, which, of course, is not to be thought of. Wartman, Sir David Brewster, Sir John Herschell, and others, have devised sundry methods of cure, which cannot for the moment be discussed. We think a series of experiments might be induced which can overcome the defect, by an arrangement of certain colored glasses operating to make up a deficiency of prismatic refraction.

Correspondence.

SYPHILITIC MALADY.

A collection of Notes on the origin of Syphilis, the different Doctrines, &c., maintained by the different European Authors—embracing a few questions to M. Ricord, with his Reply, &c., &c.

[Concluded.]

BY DR. A. C. HAYNES, PARIS, FRANCE.

DUALITY.

Joseph Grundbeck, who studied the first manifestations of syphilis in Germany, wrote, in 1496:

"This pestilence is a new species of disease that God has caused to fall upon the French as a horrible monster of which no person had ever spoken or seen,—and its existence was entirely unknown."

Alexander Benedictus, doctor in the Venetian army, which was defeated by Charles VIII., at the battle of Fornoue, in 1495, observed the first effects of syphilis. "It came from the west," says he; "it surpasses in horror, by its frightful aspect, and by its nocturnal pains, the leprosy, elephantiasis, and all the other diseases; it is a new fruit of *venereal* intercourse, unknown to the ancients, and incurable."

In 1494, King Charles VIII. invaded Italy at the head of a large army, and laid siege to Naples, defended by an army composed of adventurers belonging especially to the Spanish and

Italian nations. It was during this siege that a new disease appeared for the first time, "characterised," says Philip Beroald, who observed its first effects, "by projecting and enormous spots or pimples (taches); by pustules, which give to the face and body a hideous aspect, terminating sometimes by excessive pains in the articulations which interfere with sleep, and consume slowly the body, and no remedy can cure it."

As to blennorrhagia, the proofs for its existence among the the Romans, Greeks, and even the Hebrews, are less decisive than the simple chancre. Hippocrates says, that there exists among women "a white, yellowish flowing; when the woman urinates she experiences burning and lancinating pains; the uterus ulcerates." Celsus is not more explicit than Hippocrates. Michel Scott, who wrote about the commencement of the thirteenth century, says: "If a woman experiences a flow, and if a man has connection with her, his penis is easily altered; as it often happens to young men, who, ignoring this danger, the penis, in consequence, becomes diseased."

Tortula has described perfectly balanitis. "Subsuidena, sui in virga virili patiuntur inflammationem, sub præputio foramina multa cum excoriatione."

All the writers of the fourteenth century agree that syphilis is a *new disease*, unknown to the ancients. We should accept their opinions with great confidence, for among their names we find collected the most illustrious doctors of that epoch, searching the source of the great epidemic of 1494.

Opinions the most different, fables the most absurd, at the commencement, were invented to explain the cause of syphilis. Pierre-André Mathiole, Paracelse, Musa Brassavole, pretended that it had its source in an impure intercourse between lepers and women affected with venereal buboes. Gabriel Fallope of Modena, and André Carsalpin d'Arezzo, doctor of the Pope Clement VIII., pretended: 1st, that the Italian bakers had poisoned the bread destined for the French; 2d, that the water which they drank had been corrupted with blood and detritus proceeding from the hospital St. Lazare, situated at Somma, near Vesane.

Fioravente reported that the sutlers of the French army, as a last resource, had given to the soldiers human flesh, already in a state of putrefaction, to eat; others related that the disease declared itself after the ingestion of the meat of a farcy mare.

Roderic Diaz d'Isla, in a book dedicated to John III., king of Portugal, and which appeared about 1540, wrote the following lines: "The pox appeared in 1493 at Barcelona; this city was the first infected; afterwards Europe and all the known world. The disease comes originally from the Isle of Spain, as experience has shown; for Admiral Columbus having discovered the isle, his soldiers, who had intercourse with the inhabitants of the country, caught easily the disease, which was contagious. Christopher Columbus having arrived afterwards at Barcelona, the city was soon infected with pox, which made astonishing progress."

Ferdinand Columbus, who has described the different voyages of his father, relates that when the latter set out from Cadiz, 25th September, 1493, at the head of seventeen vessels, he arrived at St. Domingo, the 27th November the same year, and found the garrison, which he had left, reduced to half its former strength, and the one hundred and fifty men who remained were all infected with syphilis.

Gonzalve-Fernandez d'Oviedo, who had sojourned for some years at St. Domingo, where he had been sent on a mission for King Ferdinand, wrote, on his return, in 1535: "The pox is the same as in this country; but, by the effect of the Divine will, one finds everywhere the proper remedy to cure it; to wit, the wood of guaiacum: it raged also among the Chirstians but very little."

Nanclerus and, later, Gruner, pretended that the Jews, chased from Spain in 1483 by King Ferdinand, who obeying the suggestions of Torquemada, fled at first into Africa, and from there into Italy, where, by consequence of tribute, Alexander VI. permitted them to establish themselves, and where they brought the pox.

QUESTIONS AND ANSWERS.

Will an indurated chancre reproduce itself on the individual who bears it, or on a person having syphilis?

No: the law is, when a chancre is once developed, another one of the same nature *cannot* be produced on the same individual. It is the same as though he had constitutional syphilis,—in fact, we might consider the induration a secondary symptom; it is, at least, a proof of the contagion having taken place. I have in some cases inoculated a syphilitic person, and produced on him a chancre, which chancre, was not the true indurated chancre. However, if a person in perfect health, at least free from constitutional syphilis, is inoculated from this chancre, he will have a true indurated chancre. A person once having had a hard chancre which has healed, there may be left at the same time, induration under the cicatrice, though it may be scarcely perceptible from a close examination. This person may catch a soft chancre on this cicatrice, and give to another person, syphilis. The irritation of this soft chancre may give to it the appearance of a hard chancre; this is a chancere base d'imprunt (chancre mixté).

Is it necessary that the part shall be denuded of the epidermis to admit of contagion?

There must be a door of entrance, and this door must be opened before you can enter; so of the syphilitic virus, there must be a door, and this door must be opened for it to enter; with abrasion more certain is the contagion. The poison often enters through the follicles, they being well opened; viola une porte d'entrer.

The male alone being syphilitic, will the offspring be infected?

Yes.

Is abortion ever produced from the fact of the male being syphilitic?

Yes: bad seeds seldom sprout.

Will the fœtus ever give syphilis to its mother, its father being syphilitic (and never present primary symptoms)?

I certainly believe so; for I have had many proofs to convince me. There are many who do not wish to admit this doctrine; it is, however, the doctrine of many at the present time.

Both parents being infected with syphilis, can it be disputed that the offspring will not be infected?

This is a great moral question; for how do we know whether the father is a father by law or a father by nature? We have proofs, though they are exceptions, that a healthy father may have a child free from syphilis born of a syphilitic mother, and *vice versa*. I repeat it, that there are exceptions; consequently, as a general rule, the child will be infected.

Does the duration of syphilitic taint in the parents have anything to do with the infection of the fœtus?

To some degree it has; the fresher the disease the more certain the contagion. The treatment of the mother has a great effect on the fœtus (the child may be born without any symptoms).

The mother being well at the moment of conception and contracting syphilis during her pregnancy, will the child be infected?

Undoubtedly.

Do syphilitic parents ever have scrofulous children, they being exempt from the disease themselves?

Yes.

Does the duration of the indurated chancre have anything to do with modifying the poison in the system?

In general, when the hardness is considerable and resists treatment, it is the proof of a more severe case.

Is constitutional syphilis ever preceded by any primitive sign except the indurated chancre?

As a general rule, a hardened chancre; exceptionally, it may come from secondary syphilis, and especially from nurse to child, and from child to nurse.

A person once having constitutional syphilis, can he ever have the poison completely eradicated?

Yes: the proof of which is he can get it a second time. But as long as the constitution is infected, he can *not* take it afresh; that is to say, as long as the constitution is infected with syphilis

he cannot catch a hard chancre, though he can a soft one. The same law that governs small-pox governs syphilis.

Can a person be infected with syphilis a second time?

Yes.

Is constitutional syphilis contagious?

Yes.

Does a hard chancre ever produce a soft chancre, and *vice versa?*

In appearance, yes; in reality, no.

NEW YORK, March 23, 1866.

MESSRS. EDITORS:—Remembering my promise, I now, for the first time, address you, and hope to say something that may interest the profession of the West. I have been in New York but one *short* week; *oh! how short it has been* to me, who, since I began the study of medicine twenty years ago, have not had one week's respite from its cares, its labors, and its thickly clustering incidents of interest.

I, for this brief period (one week), have been free, through the great avenues of the *mind*, to bring myself in contact with the men of the profession we find in this great city. I have seen but few; but have not failed to find much to interest and entertain me, as I tried to discern the permanent traits of character, —as I studied the croppings out of peculiarities.

We will begin with *Alonzo Clark*. You will not ask, "Who is Alonzo Clark?" So well have you known him—so well, and so favorably, is he known to the whole profession of this country. I watched him closely, as I saw him sitting at the meeting of the Academy of Medicine,—a full form, a little stooped; the head sitting forward upon the anterior extremity of the neck; his black eye fixed upon whoever might arise to speak; his ear a little bent forward, evidently listening attentively for anything that might be

said of *cholera* (for this was the subject of discussion). During the course of the evening his name was mentioned as that of one capable of instructing the Academy, and an intimation given that all were anxious to hear him; it was evident to any one that Alonzo Clark had the respect and admiration of all present. At last Prof. C. arose. There was a general changing of position, a putting each ones' self in readiness to hear what he might say. And all were disappointed when, with the mildest possible expression beaming from every feature, he said: "I have only arisen to say, that it will be remembered that at the college, during the past winter, I delivered some lectures on the subject under consideration; that those lectures were now partly in print, and would all be published; and that, at the present time, and under existing circumstances, it would obviously be improper to repeat what I at that time said." This is the substance of his remarks. Each syllable was distinctly enunciated; each word separated from those preceding and following it, as it came from the lips of the speaker with unequaled modulation of voice. He sat down. All were satisfied that Prof. Clark had not re-issued that which had already been legitimately disposed of. Thus I cease to speak of one who is now past the prime of physical life, whose time and energies have been directed to the study and practice of our noble profession, and in whose soul there has never burned any of the consuming passions that trammel ones' usefulness, and mar the soul that God committed to an earthen casket, that thus it might gain contact with the external world—to gather thereby and therefrom the materials (for thought) for its development.

Prof. J. C. Dalton, jr. Tall, but well proportioned; a little awkward in his gait; on the sunny side of forty-five; sandy complexion; sandy, or reddish, coarse hair and whiskers, that knew no restraining hand in their growth; features large and not regular; a nose such as has repeatedly decorated false faces and clay pipes,—large and well defined, but not pretty;—but the face taken together with the unusual intelligence that beams through every feature (even that unsightly nose), led me to say

within myself, I have found a man such as we rarely meet,—one who is not willing, in his writings or in his teachings, to cut with downy pinion the thin air of speculation; but one who works in the mine of fact, who gathers the crude material, and builds a structure of his own; not elegant in its proportions, but massive, substantial, for all time,—here, too, is a man of whom we, as physicians, as Americans, should be proud.

Prof. Willard Parker. A fine form, erect, five feet ten inches; a steel grey growth of hair, well combed over his fine head; a pleasing and intelligent face; a good address. Armed with truth, and having justice on his side, Dr. Parker would dare anything,—is conscious of his position, confident in his knowledge; but not boastful nor arrogant. Of more than ordinary natural abilities, he has added thereto the droppings of professional lore of forty-five years accumulation; with his mature judgment he seldom blunders; with a dextrous hand he executes gracefully and neatly what his architectural genius designs.

Dr. Frank H. Hamilton, Prof. Military Surgery, Fractures, and Dislocations, in Bellevou Hospital College, is under the medium size; erect in form, and well proportioned; energetic and active; fifty years of age, or more; a devoted student of his profession. Dr. H. has written the best and most complete treatise on fractures in existence, and is now engaged with the surgical part of the history of the war,—a work that will contain much valuable information, and will show the profession of the world that, during our four years war, more important improvements have been made than during the thirty preceding. Prof. Hamilton is one of those men who is willing to devote his time to patient, laborious investigation,—a good observer himself, and one who, from the accumulated material secured by others, forms his own conclusions. He has been of much more value to the world, and will have a much better record than many other men of more native talent; his talent has not been hid, but he has made the most of it. We are indebted to Frank H. Hamilton, and must discharge our obligation by following, in our efforts to make ourselves useful, his worthy example.

Dr. Lewis A. Sayre,—six feet in height; will weigh two hundred; florid complexion; light hair; blue eyes; and looks as if he would not hesitate to enter the ring with any one who would oppose him. He is a man of strong native sense, fine mechanical genius, and with an independence of thought and action that is rare. Dr. Sayre has devoted much attention to the treatment of diseases of the joints of the lower extremities; and acting on the principle of relieving the inflammatory action by taking off pressure, has carried the principle farther than any of his predecessors in this particular branch. The purpose is accomplished by extension, and by cutting tendons and muscles, and by drawing the joint when pus is formed in and about it. The doctor kindly exhibited some cases of much interest that had been successfully treated.

With your permission I will, at another time, mention other noted professors; some among whom are Profs. Post, Van Buren, Mott, and James R. Wood. H.

Transactions of Societies.

PROCEEDINGS OF THE ST. LOUIS MEDICAL SOCIETY.

REPORTED BY JAMES W. CLEMENS, M. D.

CHOLERA.

Dr. MONTGOMERY, having finished the reading of his paper on the subject of cholera—

Dr. HAMMER remarked, that little had been said as to the causes of cholera. He had stated before, that the profession

should do all in their power to prevent an outbreak of this terrible disease, and to do this, we should know how it originates. This, he conceived, was the most important part of the discussion, but, if it was viewed rigorously, it would lead to a long discussion, and he would, therefore, confine his remarks to the most prominent points.

At a certain time in the history of cholera, the profession was divided between contagionists and anti-contagionists; but at present, it is believed that the question is definitely settled: it is not contagious, as we understand the word. We now know, through the researches of Pettenkoffer and Denbrich, that the matter which engenders cholera, is connected with the dejections from the bowels. This matter, however, is not positively contagious when just passed, but becomes so through certain surrounding circumstances. By the researches referred to, it was found that the germ of cholera existed in the rice-water stools, and even before their appearance, in the premonitory choleraic diarrhœa, and is capable of generating the poison. When this germ comes in contact with decomposing animal matter, it finds the proper materials for growth, and the poison is generated; and if any one comes in contact with this compound, or if he only inhales the air, he receives the poison in his system. If the cholera stools are poured out indiscriminately, and come into contact with animal substances, the poison will be formed, and the disease will spread. Cases are on record where the dejections of a single individual were sufficient to produce cholera, extending over a great distance. The more porous the surface of the ground—the nearer the under-ground water to the surface—the more easily the poison will be generated; while, on the contrary, the more solid and dry the under-ground, the less the probability of the development of the disease. Thus far, the disease is contagious, but the contagion is confined to a single secretion, when in contact with decomposing animal matter. With these facts before us, we should endeavor to prevent the production of the poison. This may be done by several means. As dampness is favorable to its growth, make the city as dry as possible, pay great atten-

tion to sewerage, remove all animal matter scattered over a place, and, particularly, prevent the cholera stools from coming in contact with decomposing animal matter. Quarantines will not prevent the development of the germ, when circumstances are favorable. We must kill the poison in its germ.

If these views are correct, we can do a great deal of good by advising with the City Council, and carrying out a rigorous system of sanitary police, furnishing disinfecting agents, of which sulphate of iron is one of the best and cheapest. We should not go before the public and proclaim that we are entirely ignorant of the cause of cholera, and cannot stay its progress. Fear, which invites its coming and increases its fatality, should be checked in the public mind by that confidence which comes from the thought that physicians understand the disease, and are able in some degree to control it. It is, therefore, our duty to inform the public that we do know something of cholera, and can do something to limit its deadly effects.

Dr. BOISLINIERE remarked, that he would be very glad if he could believe the theory of Dr. H. to be true, but he was afraid that it was not true. Looking at general facts, we find cholera no respector of seasons or countries: it has prevailed in all climates and in all countries. In the last epidemic of cholera in St. Louis, he saw the first or second case that occurred. This was in December, 1848. In January, '49, more cases occurred. The disease continued to increase during February, March and April. In May, after the great fire, it abated for some days, and then increased rapidly to its maximum, in July, when there were one thousand deaths. It disappeared in August, suddenly: by the middle of that month there was no case in the city. We have one important fact to notice: that the disease will rage as well in winter as in summer; it has done so in many places. If the cause of cholera arises from decomposing animal matters, then the influence of cold would be to arrest the process of decomposition and the formation of gases. Heat and moisture are necessary to the changes taking place during decomposition, and as in winter the dejections, as well as animal matter, would be frozen, he

could have no faith in the explanation given. Besides, the gas or poison had never been isolated. Other explanations had been given; viewing the cause as a gas, for instance, ozone was at one time advocated. But these theories are vague and unsatisfactory. We do not know the cause of cholera; it comes and goes, and leaves behind it no mark by which its origin can be determined. He was not opposed to proper sanitary measures, but rather, earnestly advised the strictest attention to cleanliness and regular habits. Cholera prevails extensively among the poor; less so among those paying attention to cleanliness and pure air. These latter influences will also prevent that typhoid condition which often follows an attack of cholera. The more vigorous an individual, the more powerfully will he resist disease. He should, therefore, improve the general condition by attention to the rules of hygiene. In the hospitals of Paris it was found that, of those patients affected with erysipelas who were attacked by cholera, all died, while of those affected with intestinal disease, only one-seventh died. Some cities enjoy an almost perfect immunity from the disease. This had been attributed to their being built on primary rocks. Those built on the secondary formations and alluvial, suffering the most. Dr. B. called the attention of the Society to one fact, upon which he had built a theory of his own. During the last visitation of cholera, it was remarked that those persons engaged in and about large stables escaped the disease; and the explanation which he was disposed to give of it was, that these men inhaled large quantities of ammonia. The effect of this was to liquify the blood. The great trouble in cholera is the stasis of blood in the vessels, and if a remedy could be found which would cause the blood to circulate freely, it would cure the disease. Acting upon this idea, he was disposed to favor the use of alkalies in the disease, both by the stomach and by injection into the veins.

Dr. NEWMAN objected to some points in the paper of Dr. Montgomery, especially to his remarks on the use of opium. It may be true that some of the effects of opium are not desirable, as for instance, its power of carbonizing the blood; but we know

that opium sometimes acts powerfully on the skin, and, as we wish the capillary system to be excited, it presents one of the best means to produce that result. Certain cases of cholera, unattended by vomiting and purging, had been termed cholera sicca—dry cholera. The term did not appear appropriate to him. He had met with similar cases, and had found the bowels greatly distended by an accumulation of liquid, which did not pass out; but they were, nevertheless, as true cases of cholera as those which gave external sign of the disease. The reason why this accumulation existed was, in his opinion, because the nervous energy became so obtunded, that the bowels could take no cognizance of the impression made on their internal surface. In such cases, it is really useless to give medicines by the bowels, as they are entirely unabsorbed. In 1849, he met with a case of this kind, which made a great impression on his mind. It was that of a young man, who had cholera, unattended with vomiting. Acting under the impression that the liver should be excited to action, he gave him twenty grains of calomel; no other medicine was given, fearing to excite vomiting. He also ordered extensive friction externally, and mustard sinapisms to the extremities, which were continued all night. The calomel was retained twelve hours, and then, vomiting coming on, it was thrown up unaltered, and, of course, unabsorbed. He concluded that the nervous sensibility was so weakened that the mucous membrane was incapable of exercising its usual functions, and the appearance of vomiting was but an indication, and the first effort of a returning nervous power. The external treatment, by bringing back the current of the circulation to its normal course, was most important. He believed that it was very important to rouse the liver; but, unfortunately, the disease is so rapid in its course, that the ordinary means of accomplishing this end are too slow.

Dr. HURT saw it stated by some English authors, that travelers in the rural districts of France and Switzerland, had experienced the fact, that, in the poor accommodations offered on their routes —especially in the unventilated condition of the privies—they were often afflicted with diarrhœa. Suspecting this source of the

disease to be the true one, they had avoided it, and afterwards had escaped diarrhœa in their summer tours. He mentioned this as somewhat corroborating the views expressed by Dr. Hammer, as to the cause of cholera. He thought that we were pretty well justified in concluding that the decomposition of animal matter in particular seasons, might be regarded as a prolific cause of the propagation of cholera. The circumstance under which it raged with such unprecedented violence in the season just past, was evidence of that fact. 200,000 individuals have visited the tomb of the prophet during the past season, and, aside from the very uncleanly habits of the inhabitants, their mode of living, etc., the religious ceremonies require the destruction of many animals; and the offal of these was strewn in every direction, lining the waysides and roads leading to the city. It was during the decomposition of this animal debris that cholera visited them and swept off great numbers. These matters seem to have something to do, at least, with the propagation, if not the cause, of cholera. As to the essential morbific cause, it seemed to be generally conceded that it must belong to that class of malaria which exists in the form of gas. He did not give his preference to this view. Chemistry had made us acquainted with all the gases, and almost prepared us to say, that there are no mere elementary principles that can be reduced to that form. Of the gases known, experience and experiment have taught us the influence they have on living animal matter, and whether they would produce symptoms approximative to those of cholera. No combination of gases, as yet known, is capable of producing the symptoms of cholera. As to those gases, liberated in the decomposition of animal or vegetable matter, we know those most common; viz., carbonic acid, sulphureted hydrogen and ammonia; of these, one has been claimed as a prophylactic, and if the urine of a horse will give exemption to the disease, why will not that of man? We are not so well acquainted with the families of the fungi, the cryptogamiæ, and especially of the infusoriæ. These are very numerous; the field has never been explored to its extreme periphery. We are not prepared to say that we are not the abodes of

millions of these animated things. Let us notice some of the effects of the morbific agent, without discussing what it is. It seems to exert its principal effects on the nervous system; it circulates with the fluids, and influences nutrition; it seems to break up that normal harmony that prevails between the nervous system of animal and organic life. This seems to be the very beginning of the disease, and it is in this way that the circulation of the blood is interrupted,—that innervation is impaired or perverted, he was not prepared to say which. The blood was arrested in its progress through the system, particularly in the lungs, producing a stasis, a sort of asphyxia; aeration is interrupted, and molecular action also, because oxygenation is imperfect. Hence, we have cold extremities and venous congestion. This condition is similar, in many respects, to that found in congestive fever; and it is in this condition, especially, that he proposes the use of chloroform, internally. All are familiar with the fact, that a full opiate will interrupt a paroxysm of fever. The same will hold good in anæsthesia of the cerebro-spinal system of nerves by chloroform. Under anæsthesia the organic actions are not interfered with; and if we control the cerebro-spinal system, we would aid the organic functions to re-establish their normal actions. The excitement of the nervous system must be controlled; check this excitement, or the wear and tear of the tissues alone will kill the patient. He thought chloroform could not be given by inhalation, as the system required all the oxygen the lungs were capable of appropriating; but it could be given by the stomach; and when that would not retain it, could be administered by the skin, especially that portion over the stomach.

Dr. Hammer remarked, that the suggestion of Dr. Hurt had been tried. In 1849, he saw the first of his cholera cases. The disease struck him as something entirely new; he could not arrive at any conclusion as to the cause or course of the disease. In such cases there is only one method to pursue in treatment; viz., to treat the symptoms. His views, at that time, and the views upon which he acted, were that, by some means or other, the bloodvessels of the body, especially the capillaries, were affected;

a state of paralysis existed, and, through their loss of tonicity, they yielded to the pressure from behind, and gradually became thinner, until the contained fluid was forced through. Not knowing the morbid agent influencing this condition, he only saw the consequences. The escape of the fluid was one of the causes of death. To prevent this draining of the vital fluid, he thought that any means by which contraction could be produced in the vessels would be successful. He, therefore, gave sulphuric ether, in twelve to fifteen drops by the mouth, frequently repeated, so as to act on the system at large. In addition to this, he employed another means, acting locally,—namely, ice. He ordered pounded ice to be given the patient, and, in some cases, pieces as large as the patient could conveniently swallow. At that time, ice or cold water was interdicted, and warm teas used; and, for his innovation upon the regular practice, he had been much censured. He had very good results from his method of treatment. Many of his patients died; but his statistics showed as much success as those of others using a different treatment. The causes of the disease were left entirely out of the question, as also the condition of the blood: he knew that the blood was altered by the loss of one of its elements, and his object was to prevent this. When he gave ether, he did it with a view of stimulating, not to produce anæsthesia. Sulphuric ether is a powerful stimulant, according to the dose given; and even chloroform, in small doses, acts as a stimulant. In inhalation, we have a period of excitement previous to the production of insensibility. If, in the commencement of the disease, we give the remedies in small doses, we shall produce a stimulant, not a sedative effect.

Dr. WATTERS remarked, that he had lately seen a suggestion that ice should be used along the spine as a remedy in cholera, and he rose to express a belief that the suggestion was not altogether irrational, and he would throw out some reasons which might probably elicit some discussion on the subject.

There are two systems of nerves connected with the body, the sympathetic and the senso-motory systems; the one connected with organic, the other with animal life. When a sensory nerve

is cut, and the periphery be touched or irritated, no sensibility is experienced. When a motor nerve is cut, the part to which it is distributed ceases to be under the control of the will; but when the sympathetic is divided, the functions of the part to which it is distributed are not paralyzed, but the organic actions, on the contrary, are increased. For example, when the sympathetic, passing to the ear of a rabbit, is cut, both the temperature of the part and the amount of blood in it, are increased, and the heat shown to be present goes beyond that point, which could be accounted for by the increased amount of blood. The congestion is not simply a passive congestion, such as would be supposed to follow a paralyzed condition of the nerves of the vessels, but it is of an active character; and the explanation which has been given is, that the congestion and elevation of temperature are the result of the increased molecular changes taking place between the tissues and the blood, by which heat is produced, and which changes are controlled or governed by the sympathetic system. When electricity is applied to the divided portion of the nerves, all these abnormal phenomena are removed. The point is this: that whilst the paralysis, resulting from the division of sensory and motor nerves, causes a diminution in the activity of a part, that resulting from the division of the sympathetic, causes an increase in the activity of the part; and thus those circumstances which produce sedation of the one, would cause stimulation of the other. Hence, applying these facts to the views advanced by Dr. Newman, if in cholera there was paralysis of the nerves distributed to the intestines, that paralysis would be evidenced by an over action, instead of a diminished one.

He was not aware that observations made on the dead subject gave any indications of an enlargement or extension of the blood-vessels; so, to suppose that they are enlarged by a paralysis of the sympathetic nerve, is to assert what observations do not corroborate. It is not so much, however, to account for the effusion as to account for the fact of the non-absorption of the liquid. In health, there is daily, enough liquid passing out to produce symptoms analogous to those of cholera, were it not re-absorbed.

It has been estimated that it requires eighteen pints of gastric juice to digest one pound of meat. Besides this liquid, others are drawn from the blood, whose amount is unknown; but all, or nearly all, is re-absorbed, and the equilibrium maintained. It does not follow that an enlargement of the blood-vessels is due simply to a paralysis of the nerves, which retain the blood-vessels in their normal size. There are other influences which may produce this. In the capillary system of vessels, there is not alone the influence of *vis a tergo* as a cause of congestion and enlargement, but there is also a *vis a fronti*. That the sympathetic nerve influences the size of the capillaries, is undoubted; but how does it do so? Is it by direct action, or is it by regulating those changes taking place between the blood-vessel and the tissue—the changes which produce the *vis a fronti?* Is the congestion caused by a division of the sympathetic nerve distributed to a part, due to direct action of the nerve on the coats of the vessel, or to the withdrawal of the regulating power and the consequent increased molecular changes in the tissues which follow? To explain his meaning more clearly, he gave the following example: During the inaction of a muscle, only a certain amount of blood is sent to it, and a certain amount of molecular changes take place. Owing to the influence of the will, this muscle is called into action, there is an increased flow of blood to the part, and increased molecular changes taking place in it. What is it that controls this ebb and flow of blood—this increased and diminished amount of molecular change? The peculiar province of the nervous system is to determine and regulate these changes, and the amount of blood necessary: the motor nerves increase whilst the sympathetic diminish. If this be true, it would suggest that the application of ordinary sedative agents would produce a stimulant effect,—a seeming paradox.

Dr. BOISLINIERE remarked, that it had been found, by observation, that after death from cholera the temperature of the body actually rises for several hours, showing that the chemical changes or the molecular movements from which heat results, go on after death, and thus far seems to corroborate the views of Dr. Watters.

His own opinion was, that cholera belonged to the zymotic class of diseases. The disease is never developed as a single instance: it always appears among an aggregation of persons, and travels with man, and at the same rate that he travels. Men are necessary for the production of cholera: it followed the caravans of emigrants to California, along the same road, and it follows the margins of rivers along which traveling is done. In a few instances, isolated villages have been attacked, but the source of the disease has been traced to some individuals coming from a city, or from the line along which cholera was advancing. Over-crowding seems to be essential to the production of cholera, based on the fact that cholera appears in crowds, and follows men. Being a zymotic disease, it is one of fermentation: a small amount of poison is introduced in the system, and affects the blood; therefore, the blood must be primarily diseased, and the nerves secondarily. Zymotic diseases appear and disappear with rapidity, as cholera does; as for instance, measles, scarlatina, etc., and, as in these diseases, certain accompanying circumstances may be necessary to the full development of the poison. The question is: are there any agencies that will act as antidotes, and deprive the poison of its malignity? Since his last remarks on the subject, he had made further inquiries, and found that not only men engaged in stables, breathing the ammoniacal vapors engendered there, escaped cholera, but also those employed in the manufacture of bone-black, and those engaged in the manufacture of tobacco, in which employments ammonia is inhaled. By still further inquiries he discovered that all those who worked in foundries, those engaged in smelting, colliers—all escaped cholera. Lead ore always contains sulphur, as also does coal, and in the manufacture, sulphurous acid is disengaged, and acts as an antidote. The supposition is that zymotic diseases are diseases of fermentation. Fermentation is nothing but an oxydisation, and for the proper action to take place, oxygen is essential. Remove the oxygen, or take it away from the fermenting body by substances which possess a stronger affinity for it, and you destroy the increase, if not the existence, of the fermenting body. Sulphu-

rous acid possesses the strongest affinity for oxygen, and will seize upon it to the exclusion of a fermenting body. The application of this fact is shown in the fumigation of wine casks and cider barrels, before they are filled with new wine or cider: the appropriation of the oxygen prevents the aceteous fermentation by the destruction of the ferment. By the breathing of this gas, the oxydizing power of the poison is destroyed, and it therefore acts as an anti-zymotic, and prevents the poisoning of the system by a fermentating poison. The sulphates would act in the same manner, by being decomposed in the system and giving up their sulphurous acid; and he suggested, as a happy combination, the use of the sulphate of ammonia.

Dr. BLAND would confine himself to practical ideas. As to the agent producing the disease, he confessed he knew nothing about it, and others had best come to the same conclusion. No researches—and many have been made—no chemical analysis made by experts, have informed us of any means by which we can gain an insight into the subtle and fatal agent. As to the causes in action developing this agent, we have heard much. According to Dr. Hammer, cholera is not contagious, *per se;* neither filth, from whatever source it may come, nor human dejections, under ordinary circumstances, are sufficient to produce the disease; but that dejections, when brought into contact with animal matter in a state of decomposition, developes a *something*, from which the disease arises. This seemed to him unreasonable. If this is the source of cholera, why has it not become domiciled amongst us? We have the sources constantly at work, and yet it is only occasionally that we have an epidemic. Can this explain the advent of the disease, coming like a thief in the night, unheralded, unexpected, sweeping away its thousands of mortals in a few days, and then departing as it came, suddenly, leaving nothing behind it but the newly-made graves as a mark of its deadly power? Why should it cease at all whilst there exists the source of the disease, developing anew, with each case, and gaining an access of power with each death? He did not pretend to say what was the source or what the cause of cholera.

It is, thus far, placed beyond our conception or reach. The Great Author of us all, holds it in the hollow of his hand, and we must bow in submission to His chastisements, leaving to Him that enlightenment when it pleases Him to give it.

The poison of cholera, whatever it may be, acts on all alike, yet the same results do not follow on all alike. The poor, living in squalidness and filth, crowded in small rooms, breathing, day and night, the fetid exhalations of their own bodies, seem to suffer the most. The disregard which they show to all hygienic rules, make them predisposed to an attack of any disease, and we find that it is from them, for the most part, that cholera chooses its victims and intensifies its poison. Hence, we can make use of all sanitary means to banish these sources of predisposition from our midst. Clean, not only our city, but our houses; remove the poor to more wholesome quarters, providing them with good food and pure water; and in carrying out these measures, the rule is, the improvement of our systems, making us more able to resist the disease.

In the pathology of cholera, we can find little to aid us in understanding it. Dr. Hunt had stated that there was congestion in the lungs, but he had met with cases where the lungs were unobstructed. In some, he had noticed the liver smaller than usual, and dry, the alimentary canal covered with an albuminous covering, and yet presenting no evidences of inflammation beneath. In many cases there were deep-seated congestions, but in others this seemed to be absent. There was in the latter stages a marked want of secretion. He had never noticed a patient, during the progress of cholera, shedding tears; the saliva was lessened, and there was no urine. All that we can say is, that sometimes the lungs are engorged, sometimes the liver, sometimes great engorgement of the stomach and bowels, and then again, no evidences at all of inflammation or congestion. The impression of the agent, whatever it was, seemed, first, to be centered on the nervous system—how, we know not—and the irregular actions and the variety of pathological changes due to the perversion of nervous influence.

What, then, are the best means to be adopted to lessen or avert a grave attack of this disease? Dr. Montgomery had thrown out some wholesome suggestions, and he could accord with them all, except in the prescription of opium. In some cases he thought it appropriate, in others, not. In all his cases he found it necessary to address his remedies to the nervous system, and to opening the secretions of the body. One means, addressed to the nervous system, had particularly struck him with an idea of its usefulness, viz: strychnia, in decided doses, sufficient to produce an excitation of its peculiar action. He had never used it, but believed it would be productive of good. If there should exist no evidence of congestion of the brain, although the patient be cold, pulseless and apparently dead, he would give calomel and opium in small doses, one-half to one grain of opium, and one to five grains of calomel, every one, two or three hours, as the case might call for it, with the view to unloose the secretions and change the character of the dejections. He never saw a patient recover who had not dark discharges. These remedies, in connection with stimulants to the spine, cups or sinapisms, or rapid blistering, warm, dry applications to the extremities, etc., were the best remedies, although they often failed.

Review of Current Medical Literature.

OBSTETRICS—DISEASES OF WOMEN.

1—*Placenta Prævia.* By Prof. ISAAC E. TAYLOR. (*Med. and Surg. Rep.*, 1866, No. 8; from Transact. N. Y. State Med. Society.)

"The Transactions of the New York State Medical Society for 1865 contain an elaborate paper on the *anatomy, physiology, pathology, and treat-*

ment of placenta prævia, by Prof. Isaac E. Taylor, of Bellevue Hospital, illustrated by diagrams and drawings. He concludes with the following propositions:

"1. The perfect integrity of the cervix uteri during utero gestation in its whole length, without developing from above downward, or from below upward, but modified by physiological softening to prepare it for the office of expansion at the time of labor, and not before, for the exit of the child.

"2. That the placenta prævia centralis is over the os uteri internum, and not in the cervix uteri at all, before labor commences, as is believed or demonstrated.

"3. That the limit of spontaneous detachment of the lower polar circle, the boundary line of Dr. Barnes, is not the zone of safe attachment after separation of the placenta.

"4. The cause of arrest of the flooding in general, is the limit of expansion of the os uteri internum to the extent of 12 or 14 inches in circumference, and 3½ to 4 in diameter, and 3 inches long.

"5. That the boundary line thus reached by nature is only safe so far as separation occurs by the contractions of the uterus.

"6. That the hæmorrhage comes from the uterus as seen, and not from the placenta.

"7. That the flooding is diastolic, not systolic.

"8. That the method of separation of the placenta by the uterine contractions is from the centre, and not from the margin.

"9. That the method of Dr. Simpson is preferable, in cases of extreme exhaustion, to version, until nature can be restored in some degree to perform it.

"10. That the external version should be adopted first, in transverse presentations of the shoulder, neck, or face, before rupturing the membranes; if not successful, then internal and external version together; then true version, but not rapidly, if the other methods fail.

"11. That the former and usual methods are confirmed by these investigations, in their value, such as tamponing and rupturing the membranes, and giving ergot."

2—*A Case of Spontaneous Amputation in Utero.* By O. H. Young, M. D. (*Med. Record*, March 15, 1866.)

"On the 19th of May, 1862, a daughter was born to Mr. Rodgers, of Bemis Heights, New York, with a remarkable deformity of the right leg and thigh. The leg was wanting from about four inches below the knee, and the thigh was partially amputated about four and a half inches above the knee-joint.

"There are quite a number of cases on record, both of complete and partial spontaneous amputation; but none of them are entirely similar to the present, inasmuch as it illustrates both processes, and that, too, on the same limb.

"There was nothing in the condition of the mother during her gestation to account for the deformity of the child. She experienced no shock or injury, nor was she subjected to any fright or other extraordinary mental impression during her pregnancy.

"The stump of the amputated leg was completely healed, and was perfect in shape, as though it had been made with the surgeon's knife. The extremity of the stump was covered with smooth normal skin, there being no cicatrix visible excepting a small spot in the centre.

"This peculiarity has an important bearing on the question as to the nature of the process by which spontaneous amputation is effected; and it is difficult to examine closely, and reflect upon the appearance of such a stump, without being forced to the conclusion so ingeniously advanced and supported by Dr. Montgomery; viz., that the agent by which the amputation is effected is a *ligature*. In some cases the umbilical cord is found tightly coiled around an arm or a leg, and deeply imbedded in a groove or depression in the soft parts. In other cases the child is born with a limb partially divided, and the depression found to be occupied by a cord of fibrous lymph, an adventitious material thrown out from the placenta or the umbilical cord, or sometimes from the body or members of the fœtus.

"In the case of Mr. Rodgers' child there was no abnormal appearance of the umbilical cord, nor any indications of a ligature, excepting its effects.

"The integuments were carried inwards, and drawn into a conical form, the continuity of the skin being perfect, with the exception of the small central cicatrix. The subjacent parts would thus be compressed, and after a time the blood-vessels would partake of this compression to such a degree as no longer to be capable of transmitting sufficient blood to maintain the growth of the limb below the constricted part. The bone would necessarily be involved in the process of interstitial absorption, and thus gradually reduced in size and strength, so that it would yield to a very slight force,—as, for instance, the motion of the fœtus itself. But a small spot would remain to be cicatrized, where the previously reduced bone and integuments were divided.

"The separated part of the limb was very likely present, and might have been found among the secundines; but no search was made, for the reason that the room being dark, the deformity escaped the notice of the attending surgeon, and was not known to him until his second visit was made, at which time the secundines had been thrown away.

"The *thigh* was partially divided four and a half inches above the knee-joint. The diameter of the constricted part was one and one-third inches, while the limb directly above and below measured three inches in diameter. (These measurements were made when the child was three years and five months old.)

"The depression was uniform, and of nearly equal depth around the limb. It was such as would result from the application of a ligature tightly around the child's thigh. The bone was intact, and the blood-vessels not obliterated, which accounts for the natural symmetry of the limb below the depression.

"The child was brought to Professor Armsby while very young, and he advised the parents to wait. It was brought to him again when at the age of three years and five months, when the operation was performed. A circular sweep was made with the knife, the tissues were retracted, and the femur divided. One artery was ligated, after which the flaps were united by a single suture, and the dressing completed in the usual manner.

"The stump was completely healed at the end of a week; the case having progressed in the most favorable manner.

"The amputated portion was dissected, and found perfectly normal. The tibia and fibula were closely approximated at their extremities; viz., where the spontaneous amputation had occurred, and the ends were neatly rounded off."

3—*A Case of Version by External Manipulation.* By J. B. REYNOLDS, M. D. (*New York Med. Journal*, March, 1866, p. 433.)

"Mrs. C., aged 35, fell in labor with her fourth child, having had one miscarriage. From her former physician, as well as from herself, I obtained the following account of her three previous labors:

"First, presented breech, child lost; the second, breech, child born nearly asphyxiated, but was resuscitated, and is now living; and the third child presented transverse, with its back anterior, and the head in the left side. The child was turned in the usual way, by introducing the hand within the uterus, seizing the lower extremities, and delivering; but the respiration could not be established.

"I was called at noon of November 9th, 1865, and having been forewarned that she had never had a natural labor, I went prepared for trouble. Saw her at a quarter past one P. M., and found her in labor for five or six hours, but the pains were very feeble, and at long intervals. Examining per vaginam, I found the os largely dilated, and the bag of water pouting, but could feel no presenting part. Upon introducing my hand into the vagina, and pushing up the membranes in the interval of a pain, I could feel the ribs; but, fearing lest I should rupture the membranes, I withdrew my hand to examine the abdomen externally. At this time the patient informed me that I need not fear to tell her that it was a cross-birth; for she was fully aware of it, as it was exactly like the last, where the doctor turned. The motion of the child, she complained, was very severe upon the right side, high up; and opposite to it she often felt, and could see, a large round mass, or ball, as she called it, roll up and down, causing the surface of the abdomen to stand out in bold relief. Upon palpation, I found a hard round mass in the left hypochrondrium, a soft mass upon the right side, and a hollow between. The fœtal heart was heard over the whole front of the abdomen, with summum of intensity a little to the left of and below the umbilicus. Fearing the rupture of the membranes, I concluded immediately to try to turn by external manipulation, especially as the pains were

not strong, and about ten minutes apart. Being upon the right side of the patient, and having the knees flexed and abdominal wall perfectly relaxed, I pressed down the head with the right hand, at the same time pressing up the breech with my left. By steady pressure and moulding (being obliged to desist but once by a pain), in less than five minutes, and with surprising ease, the head was lodged within the crest of the left ilium, in the pubic region, while the breech occupied the left epigastric region. The uterine mass, at this time, formed a concavo-convex shaped tumor, with concavity looking to the left. The head, deep within the crests of the iliæ, could be grasped and moved by the hands externally. She now complained considerably from the severity of the fœtal movements in the epigastrium. The fœtal heart was now most distinct a little to the right of the median line. Examining again with the hand in the vagina, by pressing up the membranes, I felt the fœtal head. The membranes were quite tough, and required some trouble in rupturing them. The os, before well dilated, contracted down to the size of half a dollar after the waters came away, having been twisted forward closely to the pubis; and the outlet for the waters seeming to have been carried up within the edges of the os, as a second bag began to pout; but by pressure upon it the liquor amnii flowed away from within the os. The head was held in place, though there seemed no tendency to slip, for over three hours, before it fully engaged within the brim. About five P. M., I thought I felt a fontanelle and suture, but could not make out the position. Two hours later, the pains still being feeble, but the head slowly descending and the fœtal heart beating regularly, I became very doubtful of the presenting part being the vertex; and at eight o'clock the chin separated, and the face soon cleared the vulvæ, the chin looking up and to the left. I had converted a transverse into a face presentation. The mother and child are doing well."

4—*Cephalic Version by Internal Manipulation.* By Dr. O. V. FRANQUE. (*Wuerzburger Med. Zeitschrift*, VI., p. 341, 1865.)

Although attention has been frequently, and by various observers, directed to the great and important advantages of cephalic version in cases of cross presentation, the operation is performed but rarely; at least, not so often as it is indicated. Many physicians do not consider an attempt at it worth their while, even when all the necessary conditions for it are present; but proceed at once to turn by the feet, and often to extract, even without a strict indication for the extraction. It does not surprise the author, therefore, that in private practice so many children are still born after cross presentation; the usually

preferable operation of turning by the head is neglected, and two distinct operations, turning and extracting, are united into one.

Now version by the head is an operation which presents essential advantages to both mother and child, over turning by the feet; even in case the head, after successful turning, has to be extracted by the forceps.

Regarding the advantages to the child, it is an established fact, that head presentations are the most favorable, whether primary, or artificially induced from a lateral presentation. F. does not deny that forceps delivery, after cephalic version, gives an unfavorable prognosis as to the life of the child; but the chances, nevertheless, are better than in version by the feet,—which he proceeds to prove by statistical figures.

To the mother, also, the operation is much more favorable than turning by the feet,—a proposition which it is scarcely necessary to demonstrate, since every obstetrician is aware how dangerous the operation of turning by the feet is, or at least can be, to the mother; but no injurious consequences to the mother have yet been reported after turning by the head. While rupture of the womb has occasionally been caused by the former, they have not, so far, occurred after the latter operation.

Finally, also, the operation is more advantageous to the operator, although F. is far from recommending that the accoucheur should be governed by his ease and comfort in the choice of an operation.

One of the reasons why this operation is so rarely practiced, in spite of its evident advantages, may be the routine of physicians, the custom to turn and extract in every case of lateral presentation. The principal reason, however, is this, that the conditions for the operation have been laid down in too restricted limits:

1. The child should be easily moveable in the cavity of the uterus, so that a change of position can be easily accomplished; the membranes should not be ruptured, or at least but little time should have passed since the rupture of the membranes. The author does not agree with this condition in its entire extent. If the obstetrician were absolutely governed by it, the operation

would be performed more rarely than is already the case; because in private, especially country, practice it is an exception to find the membranes intact. No doubt, the operation is easier before the rupture of the membranes than afterwards; but as long as the presenting part is not impacted in the pelvis, the turning is often successful. An attempt is, therefore, indicated; the more so because it cannot possibly do any harm, and, in case of failure, turning by the feet can be immediately performed.

2. The head of the child must be situated near the superior strait, so that the hand need not be introduced too far into the uterus. This condition, also, is not absolutely necessary for the execution of cephalic version; the nearer the head is to the brim of the pelvis, the easier is the operation, it is true; but it is not made impossible by a more distant position of the head. Only when the head is found out of reach,—in the fundus, perhaps,—it is preferable to bring down the breech.

3. Cephalic version is to be performed only if regular contraction of the uterus can be relied upon afterwards. This proposition the author cannot agree to. He believes that, in case of an operation in which the entire hand is introduced into the uterus, no opinion can be expressed in advance as to the subsequent contractions. It is not correct to suppose that, after turning, the head could not be fixed in the pelvis on account of deficient pains; the very initiation by the introduction of the hand will suffice to increase the activity of the uterus. But even should it be impossible to retain the head in its position, should the head, for whatever reasons, repeatedly escape into its previous position, nothing is lost by these attempts, and turning by the feet will succeed as well now as before. With some patience and perseverance, however, these attempts will seldom fail.

4. Cephalic version is not to be attempted in case of contracted pelvis. This condition also seems to limit the operation too much; the character of the labor pains and the degree of resistance of the head of the child will modify it. An attempt at turning may be indicated with a conjugate diameter of three inches. If, with a conjugate of between three and four inches,

the head is found relatively small, with wide sutures and large fontanelles,—if, after it is brought down, it is not alone retained in its given place, but also moved on by strong contractions of the womb,—turning by the head is preferable to all other modes of operation: even if extraction by the forceps becomes necessary, the results will be comparatively favorable.

5. No part of the child, nor the umbilical cord, should be prolapsed. Before the membranes are ruptured we cannot speak of a prolapsus; and the mere presentation of parts is not a contra-indication against turning upon the head. After the rupture, a prolapsed arm, especially when the hand protrudes out of the vulva, will contra-indicate the operation. But if only a part of it presents (an elbow or a hand), which is not impacted, the author can see no obstacle; for the operation has been actually performed with success (by *Trefurt*) under such circumstances. In a prolapsus of the cord F. recognizes no contra-indication to the operation; the accident is not rare in lateral presentations, but its prognosis in this case is not nearly so unfavorable as in vertex presentations. He cannot appreciate the advantage which turning by the feet is said to have, in such a case, over turning by the head; for the very greatest danger in breech presentations is the pressure exercised necessarily by the head upon the umbilical cord. After a successful turning by the head, there can be no prolapsus of the cord; for with the entire hand in the uterus its reposition must have succeeded.

For all the above reasons, it is desirable that the conditions for attempting cephalic version should not be confined into too narrow limits. The contra-indications against the operation are stated as follows: 1. Anomalous form of the uterus (when it can be diagnosticated). 2. Death of the fœtus; because the operation is performed chiefly with the view of saving the life of the child. 3. Version by the head is absolutely contra-indicated in all cases, where, for whatsoever reasons, acceleration of labor is necessary.

5—*Induction of Premature Labor.* By Prof. G. T. Elliot. (*Med. and Surg. Reporter*, 1866, No. 7.)

At the meeting of the New York Academy of Medicine, held February 7th, Prof. G. T. Elliot read a paper on the Induction of Premature Labor. While the justification of premature labor, under certain circumstances of parturient eclampsia, albuminuria, and pelvic deformities is now generally admitted, and the history of the operation accessible to all, it must be conceded that the *clinical* difficulties are not yet all removed. With a view of throwing some light on these difficulties, and of calling out the unrecorded experience of the members of the Academy, he would contribute the results of his clinical experience.

The main point which must be borne in mind is, that the greater or less rapidity with which premature labor, or labor generally, may be induced, depends upon the greater or less dilatation, or *dilatability*, of the cervix uteri. The actual dilatation of the os and cervix is frequently of less importance than the *dilatable* condition of the parts; and operative interference may be practicable when there is little actual dilatation, but a fair degree of dilatability.

Several cases occurred in the clinical experience of the author, where the cervix being found sufficiently dilatable, small forceps were introduced within the cervix, and by gradual traction, delivery effected without any ill consequences upon mother or child.

The number of clinical observations embodied in Dr. Elliot's paper is over thirty.

Regarding the measures resorted to, to induce full dilatation or dilatability of the cervix, there are chiefly four: The *douche*, *Barnes' dilator*, *sponge and other tents*, and *manual* or *digital* efforts.

The *douche* has very rarely failed to induce dilatation of the os and cervix, and no evil results have been observed to follow from it. In four cases it was resorted to alone, no other means being necessary to produce the desired effect. The number of douches applied, and the time from the first applications until delivery in these cases, were as follows:

1st	case:	2	douches,	30	hours.
2d	"	2	"	31	"
3d	"	2	"	16	"
4th	"	3	"	16	"

The last case was one of eclampsia from albuminuria, and at the application of the first douche everything was as unlike labor as imaginable, cervix hard and undilatable, vagina dry, etc.; yet labor was terminated successfully in sixteen hours, by the application of three douches.

The proper application of the douche in inducing premature labor, need not be accompanied by any dangers. To be effective, the stream of water injected should be directed against the internal os, and if possible, within the os. A finger introduced in advance of the stream will serve as a guide.

The method by Barnes' dilator is more widely useful than the douche, and failure in its use need hardly ever be expected. It is, however, always necessary to bear in mind, that this, like every other operative procedure in midwifery, must be carefully and properly conducted.

Sponge tents (Mason's), and others, may be necessary to commence with, in dilating the os and cervix.

An important point, in the clinical experience of Dr. Elliot, regarding the induction of premature labor, is the early separation of the membranes, which may be accomplished by the introduction of a male catheter into the uterus, between it and the membranes. But great care must be taken that the instrument be introduced with sufficient caution not to rupture the membranes prematurely, and thus deprive the operator of one of the most powerful natural stimuli of uterine dilatation.

Cases were detailed by Dr. Elliot in illustration of every important point paper of his ; and in conclusion, he expressed his opinion, not only that the induction of premature labor would, if its clinical relations were better understood, be selected more frequently and uniformly in certain conditions threatening mother and child; but that it would be so deprived of its dangers and fallacies, that it might be resorted to *as a matter of* CONVENIENCE.

6—*On Vaginal Hæmorrhage during Pregnancy, Labor, and the Puerperal Period.* By Dr. O. V. FRANQUE. (*Wiener Med. Presse*, 1865, No. 47.)

Spontaneous hæmorrhages, the source of which is in the vagina, are upon the whole not very frequently met with during pregnancy, labor, or the puerperium. They owe their origin to the rupture of a vein, (unless they are—a still rarer accident—the consequences of foreign bodies in the vagina, a case which we leave out of consideration for the present; we likewise disregard in this place those hæmorrhages which have been caused during labor by extensive rupture of the vagina).

In the affection under consideration, the blood either escapes outwardly, directly upon the rupture of one or more vessels taking place; or if a deeper seated vessel is ruptured, a tumor is formed in the labia, and in the walls of the vagina, which is generally known by the name of thrombus vaginæ. (*McClintock* calls this blood-tumor pudendal hæmatocele.)

All authors probably agree at present that the blood escapes chiefly from veins, without asserting, however, that arterial vessels *cannot* participate, which happens but very rarely. The occurrence of such a hæmorrhage, or the production of a thrombus, during labor and immediately afterwards, as well as in the puerperal period, is sufficiently explained by the considerable obstruction of the venous circulation in connection with excessive dilatation of the genitals during the passage of the child. (When the affection shows itself in the puerperium, its origin must, nevertheless, be referred to the time of labor.) During pregnancy, however, we cannot speak of such

a dilatation, and the venous stasis, also, is not as considerable as during labor, although this agency must not be left out of consideration altogether. We find the cause of these hæmorrhages, in the period of pregnancy, in the greater fullness of the blood vessels of the pelvis, where some unusually thin-coated vessel can no longer withstand the increased pressure of the blood. We believe that we must assume such a predisposing thinness of the vascular coats, because hæmorrhages from ruptured veins must else be more frequent during pregnancy than they are really observed. Thrombus vaginæ, and spontaneous bleeding from ruptured veins of the vagina, during pregnancy, are among the rarest occurrences. Most frequently this affection occurs during labor, or rather immediately upon the expulsion of the child; but it is, in either case, one of the most perilous accidents, and demands, as such, the energetic interposition of art.

Regarding the state of the vessels upon which this affection is based, an opinion still prevails generally, which is by no means confirmed by practice. It is asserted by many that the anomaly in question is favored by a varicose dilatation of the vaginal veins, although the fallacy of this view has been proved by high authorities. *Scanzoni* has, in eight cases of thrombus vaginæ, observed in but one woman a varicose degeneration of the veins, and that not very pronounced. In thirty-eight cases which *McClintock* collected, varicose dilatation of the veins occurred only twice.

Varicose degeneration of the veins, moreover, is observed chiefly in women who have born several children; yet *McClintock* found among twenty-five cases of thrombus vaginæ, thirteen women who were pregnant the first time. *McClintock* treated a large number of pregnant patients with varicosity of the veins of the vulva and vagina, without observing the pudendal hæmatocele in one of them. He is even inclined to draw from these circumstances the opposite conclusion, viz: that varicosity of the veins prevents the occurrence of thrombus vaginæ.

The prognosis of these hæmorrhages, and of a thrombus of the vaginæ, is always dubious; the patients may either succumb to the great loss of blood, especially when a large superficial vein has burst, and the blood is poured out immediately on the surface; or, in case of formation of a thrombus, they may perish in consequence of ichorous decomposition of the collected blood, or of extensive tedious suppurations and burrowing of pus. The treatment, of course, is very difficult according as we have before us a pregnant patient, a woman in labor, or one just delivered.

If in the course of pregnancy a serious hæmorrhage takes place, it is, in our opinion, best to tie the bleeding vessel, in case it can be reached, as has been done [in one of the author's cases.—Ed.]. The ligature certainly prevents a repetition of the hæmorrhage, and is least disturbing to the course of the pregnancy. In case the hæmorrhage is not very profuse, the use of cold may be sufficient,—injections of cold water, introduction of small pieces of ice into the vagina, in connection with the various astringents; but in cases of very profuse hæmorrhage from a larger vessel, cold and astringent remedies will not answer the purpose. If the place of bleed-

ing is high up in the urethra, or if the bleeding vessel is not superficial, so that it is impossible to ligate it *en masse*, the only remaining resource is plugging of the vagina; but in this case it is always necessary to be prepared for another effect of the tampon, viz: the induction of premature pains, and an interruption of gestation.

If a thrombus forms during pregnancy, we should confine ourselves to the application of cold, so long as it does not increase in size. If it is certain that no more blood is extravasated, it is, in our opinion, best, unless the tumor is very small, to open it and remove the coagula. These thrombi, during pregnancy, however, seem inclined very soon to open spontaneously, at least such was the case in the majority of the observations known to us; nor does either the artificial or the spontaneous opening appear to have any deleterious influence upon the course of the pregnancy.

During labor, a thrombus vaginæ, or a *profuse* hæmorrhage from a ruptured vein of the vagina, demands immediate artificial delivery. The thrombus is to be previously laid open only, if the still increasing tumor forms a mechanical obstacle to labor. These cases are perhaps the most dangerous: the loss of blood can be so considerable that the woman dies undelivered; not a few such cases have been reported. Delivery by art must, therefore, be resorted to as quickly as possible.

After delivery the thrombus demands the use of cold and of the tampon so long as the tumor increases. As a matter of course, the state of the uterus must not be overlooked, in order to obviate the occurrence, during the *tamponnement* of the vagina, of an escape of the blood into the insufficiently contracted uterus. When the tumor no longer increases in size, it is best to open it by not too small an incision, and to plug the vagina, and perhaps the cavity of the emptied thrombus itself, for the prevention of a new hæmorrhage. Subsequently it is chiefly necessary to maintain the strictest cleanliness, by frequent injections into the vagina, under which simple treatment the cavity usually closes pretty rapidly. At all events, the duration of the affection is shorter if the thrombus is opened by art than when left to itself. In the latter case tedious suppuration and ichorous decomposition always ensue, to which, not seldom, are added pernicious forms of puerperal fever.

7—Rupture of the Womb, produced by the Administration of Ergot. Dr. F. H. MILLIGAN, of Wabashaw, Minn., (*Richmond Med. Jour.*, March, 1866, p. 199).

Dr. Milligan reports a case of rupture of the womb, produced, as he thinks, by the undue contractions excited by ergot, which a midwife administered "during twenty hours" for "sluggish pains." The original obstacle to labor seems to have been contraction of the superior strait, from which the woman had experienced some difficulty in her former labors.

MATERIA MEDICA AND THERAPEUTICS.

1—*On the Therapeutical Action of Arseniate of Antimony in certain Affections of the Heart and Lungs.* By Dr. LUCIEN PAPILLAUD. (*Schmidt's Jahrb.* cxxix, p. 155; from *Gaz. de Paris*, 1865.)

The cases in which the author successfully employed the arseniate of antimony, usually one half milligramme a day, were in part affections of the heart, (mostly, but not always, consequent upon rheumatism or neuralgic,) and partly phthisical affections. Cases of palpitation are related, with or without valvular disease, and often accompanied with chlorosis and pulmonary catarrhs, in which the remedy was used with the greatest success. Other cases confirm the recent observations of Dr. *Wahu*, in his own case, of the favorable influence of arsenic in tuberculosis.

The author considers himself justified in the following conclusions:

1. The treatment by arseniate of antimony corresponds in its success to the usual treatment by arsenic; but in combining it with the energetic antimony a smaller dose of the arsenic is required.
2. This treatment often affords relief, and even effects a cure in cardiac affections, which have been treated without success with various other remedies.
3. Even in cases of demonstrable organic disease of the heart, the arsenic-antimony treatment effects an improvement in the functional activity.
4. The specific influence of this treatment upon the action of the heart at the same time strengthens the entire organism.
5. Arseniate of antimony is borne for years without harm,—on the contrary it is favorable to health.
6. This remedy proves temporarily, or permanently, effective against pulmonary phthisis, mainly, as it seems, by elevating the strength of the organism.

2—*The Therapeutical Use of Narceine.* By Dr. A. EULENBURG. (*Deutsches Archiv f. klin. Med.*, I., p. 55.)

Narceine, one of the alkaloids of opium, has been set down in most text books of Materia Medica that mention it at all, as nearly inert. A series of experiments by Dr. Eulenburg, of Greifswald, on surgical patients, and also on individuals in

health, yielded very different results. He remarks that *Claude Bernard*, in his experimental researches on the alkaloids of opium, found narceine superior to all the others in soporific effect, and that an overdose kills animals without producing convulsions. *Debout and Béhier* subsequently found that, when injected subcutaneously, narceine produced the same calming and soporific effect as an equal dose of morphine, without the inconveniences of the latter, as headache, sickness, impaired digestion, and syncope. These results induced the author to institute experiments, (using internal doses of $\frac{1}{6}$—$\frac{1}{2}$ gr., hypodermatic doses of $\frac{1}{8}$—$\frac{1}{4}$ gr.,) which led him to the following conclusions:

1. Narceine acts, internally as well as hypodermatically, as a *sedative and soporific*, in an equal dose, with equal certainty and precision, as morphine.

2. In this action narceine presents valuable advantages; it causes neither symptoms of irritation, nor disturbances of digestion; the sleep produced by it is profound, quiet, and the awakening perfectly clear.

3. In a number of cases, morphine is not borne, or remains ineffective from the first, or after repeated use, while narceine here also does good service.

4. Narceine acts as a *local anæsthetic to sensitive nerves* in the same manner as morphine and atropine, and displays in (peripheric) neuralgia a very satisfactory effect,—not only palliative, but also curative.

5. It appears likewise to exert a favorable influence in *spastic and convulsive neuroses*, and is preferable in these cases to morphine on account of its physiological properties."

The present high price of the remedy is alluded to as a barrier to its general introduction.

3—*The Active Principles of Veratrum Viride.* By CHARLES BULLOCK. (*Amer. Journal of Pharmacy*, March, 1866.)

The object of the author's investigation was chiefly "to obtain information whether the alkaloid obtained from the plant was identical with veratria, and whether the resinoid of Prof. Percy contained any proximate principles to which its great activity could be attributed."

As a summary of this investigation, we find—

1. That Veratrum viride contains two alkaloids, one soluble in ether, and the other insoluble in that menstruum. Neither of these principles answers in its chemical reactions to veratria.

2. That the resinous matter which precipitates when the concentrated tincture of the root is poured into acidulated water, carries down with it a notable portion of the alkaloid insoluble in ether.

3. That the alkaloid insoluble in ether, when administered in small doses, has a very perceptible sedative effect on the circulation, without producing any other disturbance.

4. That the pure resin produces sedative effects nearly or quite equal to those obtained from the alkaloid.

4—*Phenic Acid; its Therapeutical Use and Hygienic Relations.* By Dr. LEMAIRE, Paris, and Dr. STUNDE, St. Petersburg. (*Med.-Chir. Rundschau*, 1866, I., p. 17; from *St. Petersb. Med. Zeitschrift*, 10. Hft. 1865.)

Phenic acid, (carbolic acid,) a product of distillation of mineral coal, is, in the pure state, a colorless crystalline mass, somewhat soluble in water (1 part in 20), more easily in alcohol, ether, essential oils, and vinegar. Commercial phenic acid is a yellow or light brown fluid, containing 4—6 p. c impurities. It is allied to creasote and to benzine.

Phenic acid possesses the property, even in very small quantities (1 : 1000 aq.), of cutting short all fermentative processes. In solutions of 1 : 100 it acts as an energetic poison upon all the lower animal organisms; hence the idea of using it to destroy parasites, and as a disinfectant in all diseases based on miasms and contagion. *Lemaire* reports favorable results from the use of it in the destruction of mould in barns and cellars, etc.; in killing insects injurious to plants, bed-bugs, roaches, etc. Moreover, he is of opinion that in phenic acid we have discovered the very best disinfectant. The usual disinfectants absorb, remove, or decompose the products of putrid fermentation, (the great general source of miasms and contagions,) but have no power to arrest the latter. Phenic acid arrests putrefaction and prevents its renewal. Added to this, its manufacture on a large scale is so easy and cheap, that such establishments as tanneries, soap-factories, etc., which stand most in need of it, can make their

own supply, (which need not be very large, since the remedy need not often be renewed like other disinfectants).

In case of epidemics, L. recommended all dead bodies to be washed in water saturated with phenic acid, or temporarily embalmed by injection into the arteries. Injections of an aqueous tincture of coal tar have the same effect. He recommends the solution of the acid in the treatment of infected wounds, and relates illustrative experiments and cases.

Meat kept at ordinary summer heat in a room, in an atmosphere impregnated with phenic acid, showed, at the end of eight days, not the slightest trace of putrescence.

For the purpose of disinfecting hospitals, the addition of a small quantity of the acid to the lime used for whitewashing is recommended.

The therapeutical uses of this remedy are the following:

1. As a *rubefacient:* Applied to the skin, in a concentrated form, it occasions at once vivid pain, and reddens the skin for fifteen or twenty days, without, however, causing inflammatory symptoms. A solution in an equal quantity of alcohol acts less intensely, like mustard, but the effect lasts longer.

2. Against *parasites:* A solution of 1 : 100, used as a wash, kills lice and morpiones in a few minutes. Used in scabies, it effected a complete cure by three washings, with the same solution, in a patient affected with it for three months, and over the entire body. The solution, clear and colorless, also has the advantage over the usual remedies of saving the linen. In tinea capitis and herpes tonsurans, the above solution likewise proved efficient after washing the head with a decoct. saponariæ, without the previous painful depilation which is usually instituted in the treatment of tinea. In a charitable institution, the object of which is to receive children with scurf, with whom all efforts at treatment have failed, fourteen or sixteen of twenty-two patients were completely cured, according to the report of the Superior, by the method of *Lemaire*, the remainder still being under treatment.

3. In various *chronic affections of the skin*, chronic eczema, pemphigus, phenic acid was also employed to great advantage. L. also reports the remarkable success of the same remedy, in a case of ulcerating cancroid, which, resisting all treatment, had existed for eight years, over the right eyebrow of a lady seventy years of age. The wound measured three cm. in length, and two mm. in depth, and cicatrized, under the use of ablutions, morning and evening, with a weak solution (1 : 1000) of phenic acid, and the application of compresses saturated with it, in twelve days. In offensive, gangrenous ulcerations of the soft parts, and the ulcers accompanying

caries and necrosis, phenic acid has also been employed, not only in France, but also in England, by *Thomas*, *Turner*, and *Heath*, by the latter, however, in much stronger solutions (1 : 40); and not only is its efficacy attested in disinfecting the wounds, but also in promoting the suppuration, the exfoliation and removal of the affected parts of the bone. In France, L. used, in similar cases, the coal tar "*saponine*," which owes its virtues to phenic acid, as a dressing.

4. As an *anthelmintic*, against ascarides in the form of enema, with brilliant success. The dose was 25 to 50 centigrammes to 125 grammes of water, and two injections in twenty-four hours sufficed to relieve the patients completely. But used as a drink, in the proportion of 1 : 1000, against tænia, it was found unsuccessful.

5. As a dressing in all *infected wounds*.

6. Without experience on the subject, *Lemaire* proposes the question, whether the destructive effect of phenic acid upon all infecting substances might not prove useful in pustula maligna, and the poison of hydrophobia. Syphilis, which he has not yet included in the range of his researches, might also belong to this category. Moreover, he feels obliged to recommend the internal use of the remedy, against all miasmatic diseases, as swamp fevers, yellow fever, cholera, plague, the various acute exanthemata,—finally, in the numerous epidemics of this character among animals, in which he proposes its internal as well as external use, to stay the rapid progress of the epizoöty.

Finally, L. recommends the application of phenic acid to anatomica-purposes, for the preservation of subjects and anatomical preparations.

5—*Caustic Collodion against Warts*.. By GIOVANNI FINCO. (*Med.-Chir. Rundschau*, 1866, I., p. 81, from *Gazz. Lomb.*, 1865.)

The author confirms in this communication the efficacy of caustic collo dion against warts. The collodion has here no other function than to suspend the sublimate, and retain it upon the spot where its effect is desired. In a person aged fifty-six, who complained of burning sensations in the anus and vicinity, and had been repeatedly treated for it, F. found the pain to be caused by a large number of warts of different sizes and shapes, partly simple, partly united into cauliflower-like excrescences, which, according to the patient's report, were syphilitic. Other symptoms being absent, F. resolved upon the local treatment of the affection. He mixed 25 centigrammes of corrosive sublimate with 2 grammes of collodion, and applied a portion of it, by means of a camel's hair pencil, to two of the largest warts, the size of a fruit-kernel (?). On the following day he found these two destroyed, then touched two more, afterward three or four, always with the same success, and ordered sitzbaths. Thus he destroyed, within sixteen days, more than sixty condylomata, which did not return, either in that vicinity or elsewhere.

6—*Flavoring Cod-Liver and Castor Oils.* By M. JEANNEL. (*Richmond Med. Jour.*, Feb., 1866; from *Jour. of Pract. Med. and Surg.*)

Therapeutics are indebted to M. Jeannel for an improvement which will facilitate the always difficult exhibition of fish oils. Like M. Sauvan of Montpelier, M. Jeannel observed that the addition of ten grains of essential oil of bitter almonds to three and a quarter ounces of the most offensive of cod-liver oil entirely dispelled its nauseous odor and fishy flavor. It was, however, not enough to disinfect the oil, it was further desirable to render it agreeable, an object which M. Jeannel has effected by very simple means; the cod-liver oil should be powerfully shaken in a phial with once or twice its volume of distilled cherry-laurel water, and the two fluids afterwards separated, after forty-eight hours rest, with a funnel. The oil should be filtered if it has not spontaneously clarified. Brown, fetid oil acquires by this simple process an agreeable perfume and a pleasant flavor of almonds, which remains on the taste during the whole time of digestion.

With regard to castor oil, three drops of essential oil of almonds communicate to three and a half ounces of oil a pleasing perfume and taste. The *Gazette des Hôpitaux* states that for several months Dr. Rennes of Bergerac, has been in the habit of successfully palliating the noxious flavor of fish and castor oils, by merely adding one drop of essence of bitter almonds to every six or eight drachms of oils.

7—*On the Rapidity of Absorption after Subcutaneous Injection.* By DR. EULENBURG. (*Med.-Chir Rundschau*, 1865, iv, 960, from *Centralblatt f. d. med. Wiss.*)

Eulenburg made experiments to determine the rapidity of absorption of substances subcutaneously injected, by their appearance in the parotid saliva, and the secretions of the month. He sometimes employed the method of *Eckhard*, obtaining the paretic secretion by introducing tubes into the oral orifice of both Stenonian ducts; at other times he made use of the entire combination of secretions of the mouth. He experimented first with iodide of potassium, and detected it in the saliva, at the soonest after one minute, at the latest after five minutes; and the sooner, the nearer to the head the injection was made. The remedy could still be distinctly detected after twelve to thirty-six, very seldom after forty-eight, hours. After internal administration it could not be demonstrated until after twenty to forty-five minutes, and the elimination was correspondingly slow after internal use. Sublimate (gr. 1-6 to gr. 1-12) could be detected in the secretions of the mouth within from two to ten minutes after injection. After internal exhibition of an equal dose of sublimate, so rapid an absorption was never observed.

Errata.—On page 166 of last number, read dr. j; dr. jj; fl. dr. ss; fl. oz. viii.

Scientific Department.

UNDER THE DIRECTION OF BENJAMIN F. SHUMARD, M. D., ST. LOUIS.

METEOROLOGY OF MARCH, 1866.

BY GEORGE ENGELMANN, M. D., ST. LOUIS.

	MARCH.	
	1866.	Average.
Barometrical Pressure—		
Mean	29″.645	29″.487
Highest	30″.078	30″.305
date	8th.	5th, 1848.
Lowest	28″.895	28″.516
date	20th.	28th, 1859.
Temperature		
Mean	41°.8	44°.4
Highest	81°.5	86°.0
date	20th.	29th, 1842.
Lowest	—14°.0	—0°.0
date	17th.	3d, 1848.
Evaporation	4°.4	5°.2
Relative Humidity	65.8	60.6
Rain—		
Quantity	2″.80	3″.81
Days on which it fell	7	8
Thunderstorms	1	3
Principal Winds	NW.	W. & SW.
Cloudiness	5.7	4.6
Fair days	3	12
Variable days	20	14
Days without sunshine	8	5
Stage of River	13-20½ feet.	14

March was this year more a winter than a spring month, with higher atmospheric pressure and lower temperature than usual; more dark and fewer clear days; and with much greater humidity, though less rain than in other years. The public health began to improve, and with the exception of affections of the respiratory organs, no diseases prevailed, The city was almost entirely free of eruptive diseases, generally prevailing at this season.

ATMOSPHERIC ELECTRICITY, TEMPERATURE, AND HUMIDITY.

Observations made at St. Louis, Missouri, based on daily observations at 6, 9, 12, 3, 6, and 9 o'clock, from morning till night.

BY A. WISLIZENUS, M. D., ST. LOUIS.

I.—*Monthly Mean of Positive Atmospheric Electricity.*

MARCH.

YEAR	6 A. M.	9 A. M.	12 M.	3 P. M.	6 P. M.	9 P. M.	MEAN OF MONTH	NO. OF THUNDER STORMS.	PREVAILING WINDS.
1861...	12.5	12.0	9.8	8.1	9.0	8.0	9.8	3	W. & SE.
1862...	11.8	12.8	9.4	8.4	7.0	6.8	9.4	4	W.SE. NW.
1863...	16.8	15.5	16.5	10.4	11.1	12.3	13.6	2	NW. W. NE. SE.
1864...	14.2	13.7	11.6	6.8	8.3	11.1	11.0	1	SE.
1865...	8.5	9.7	6.3	4.4	3.2	3.0	5.9	4	SE. W. & S.
1866...	5.5	7.7	5.2	5.1	5.2	5.3	5.7	2	SE. NW. N.
Mean.	11.3	11.9	9.6	7.2	7.3	7.7	9.2	.....	.....

2.—*Monthly Mean of Temperature, Fahrenheit.*

MARCH.

YEAR.	6 A. M.	9 A. M.	12 M.	3 P. M.	6 P. M.	9 P. M.	MEAN OF MONTH.
1861.....	37.1	41.1	48.2	51.6	47.6	43.4	44.8
1862.....	36.0	40.1	46.9	48.0	45.6	42.5	43.2
1863.....	36.7	40.9	47.5	49.5	46.1	42.3	43.6
1864.....	34.1	38.7	44.6	45.9	42.2	38.6	40.7
1865.....	38.9	43.8	50.3	52.5	49.7	45.0	46.7
1866.....	33.5	40.9	46.4	47.8	44.3	40.6	42.2
Mean.....	36.0	40.9	47.8	49.2	45.9	42.1	43.5

3.—*Monthly Mean of Relative Humidity.*

MARCH.

YEAR.	6 A. M.	9 A. M.	12 M.	3 P. M.	6 P. M.	9 P. M.	MEAN OF MONTH.
1861.....	83.3	63.7	58.9	52.4	58.9	70.1	64.5
1862.....	85.1	76.2	64.4	60.4	65.5	78.0	70.8
1863.....	87.7	71.5	57.8	55.4	63.0	73.0	68.1
1864.....	85.6	69.5	59.3	58.8	68.8	78.1	70.0
1865.....	81.8	67.2	59.8	55.5	60.2	72.2	66.1
1866.....	83.5	69.9	60.6	60.3	65.7	74.6	69.1
Mean.....	84.5	69.7	60.1	57.1	63.7	73.5	68.1

Editorial Department.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

A Treatise on the Principles and Practice of Medicine, designed for the use of Students and Practitioners of Medicine. By Austin Flint, M. D., Professor of the Principles and Practice of Medicine in the Bellevue Hospital College, and in the Long Island College Hospital; Fellow of the New York Academy of Medicine, &c., &c. pp. 866, 8vo. Philadelphia: Henry C. Lea. 1866. For sale by James M. Crawford, No. 54 Fourth Street.

Dr. Flint's work is as good as most essays of the sort. We cannot, however, regard it as an improvement on Watson's Practice, which has been in vogue these twenty years past. Indeed, all the works on the principles and practice of medicine are very much alike; first, some general considerations on the elements of disease—the hyperemias, anemias, hypertrophias, atrophias, &c.; then come the special diseases, that is to say, the inflammations of the various organs, and the fevers, and so "ends their catechism."

We never expect to read another work on the practice of medicine. We may, as we have done with this volume, glance at the first chapters,—perhaps read a whole chapter, but the whole work we shall not read; we know beforehand that it would not pay. Nevertheless, for the student and young practitioner this work would do just as well as any other. Dr. Flint has been a student, and his work is, we think, "*au niveau*" in regard to the present state of medical science. L.

Obscure Diseases of the Brain and Mind. By Forbes Winslow, M. D., D. C. L. Oxon. Second American, from

the third revised English edition., pp. 483, 8vo. Philadelphia: Henry C. Lea. 1866.

This is an interesting, though not very methodical work. It contains many useful and interesting suggestions in regard to insanity; and the views of the author are, we think, mainly very reasonable. The history of cases of insanity, some of them by the patients themselves, constitutes a considerable portion of the volume.

Without entering into the merits of the work, we recommend it to the medical profession as worthy of a perusal.

L.

THE PREPARATION AND MOUNTING OF MICROSCOPIC OBJECTS. By THOMAS DAVIES. New York: Wm. Wood & Co. (No year.)

A neat duodecimo of 144 pages, which in seven chapters imparts very necessary and useful information to beginners and amateurs, regarding the preparation and preserving of microscopic objects. The author's attention is directed chiefly, it seems, to the preserving and mounting of "beautiful" objects, and the book is therefore to be recommended most to those who resort to microscopy for amusement. To the natural history student, to the physician, etc., the chapters on Preservative Liquids, on Sections and Dissection, and on Injection, are of little use; whereas the chapters on apparatus and on mounting "dry," or in Canada Balsam, will be found instructive to all who desire to preserve their preparations. A singular omission occured to us in the remarks on mounting in balsam; the author represents perfect dryness of the object a most necessary condition, but forgets to mention in its proper place the method by which an object which will not bear drying can be freed from water; probably no method for mounting delicate animal tissues in balsam is oftener practiced and more widely applicable than Clarke's (the successive treatment with alcohol and oil of turpentine), especially in combination with the method of coloring by carmine, as recommended by Lister and Turner (*Microsc. Jour.*, Oct. 1859.) The want of illustration will be felt by every reader; a few wood-cuts would

have greatly enhanced the utility of the book, the *extérieur* of of which is creditable and inviting. (We regret that publishers still continue, occasionally, to throw suspicion upon their publications by suppressing the date.) G. B.

BOOKS AND PAMPHLETS RECEIVED.

A MANUAL OF THE PRINCIPLES OF SURGERY, BASED ON PATHOLOGY; FOR STUDENTS. By WILLIAM CANNIFF, Licenciate of the Medical Board of Upper Canada; M. D. of the University of New York; M. R. C. S. England; formerly House Surgeon to the Seamen's Hospital, New York; late Professor of General Pathology, and the Principles and Practice of Surgery, Univ. Victoria College, Toronto, C. W., etc. Philadelphia: Lindsay & Blakiston. 1866. For sale by Jas. M. Crawford, No. 54 Front St.

THE TRANSACTIONS OF THE AMERICAN MEDICAL ASSOCIATION, INSTITUTED 1847; VOL. XVI, 1865. Philadelphia. 1866.

This is a volume of 869 pages, containing the minutes of the sixteenth annual meeting of the Association, with address of the President, N. S. Davis, M. D., and numerous reports, prize essays, etc., etc. It should be read by every member of the profession.

Address Wm. B. Atkinson, M. D., Philadelphia, Pa.

PROCEEDINGS OF THE AMERICAN PHARMACEUTICAL ASSOCIATION AT THE THIRTEENTH ANNUAL MEETING held in Boston, Mass., September, 1865; also, THE CONSTITUTION AND ROLL OF MEMBERS. Philadelphia: Merrihew & Son, Printers, No. 243 Arch Street, below Third Street.

Pharmaceutical associations and colleges have a place of importance scarcely less vital to the interest of communities than colleges of medicine and surgery. We cordially recommend these Proceedings to pharmaceutists and apothecaries, every one of whom should have a copy in his library. It is a volume of 260 pages, and contains twenty-two very excellent special reports by—

Wm. Proctor, jr., on Liquidamber Styraciflua and its Balsamic Resin;

J. M. Maisch on the Active Principle in Phus Toxicodendron;

E. C. Bedford on Heating Apparatuses;

E. C. Jones on the Seeds of Cimicifuga Racemosa; and others of equal interest; with nine volunteer reports and essays, the minutes, constitution, etc.

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President: Henry W. Lincoln, Boston, Mass.
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Cor. Secretary: P. W. Bedford, New York.

SURGEONS OF NEW YORK. By SAMUEL W. FRANCIS, A. M., M. D. New York: John Bradburn. 1866.

This is an excellent little volume of 220 pages, containing biographical sketches of sixteen of the most prominent surgeons, viz: Valentine Mott, W. H. Van Buren, Alfred C. Post, Frank H. Hamilton, John Murray Carnochan, Jas. R. Wood, L. A. Sayre, A. B. Mott, J. P. Batchelder, A. H. Stevens, Willard Parker, G. Buch, J. Swinburne, J. S. Thebaud, Stephen Smith, and Alexander E. Hosack.

REPORT OF THE BOARD OF TRUSTEES OF THE MASSACHUSETTS GENERAL HOSPITAL, FOR THE YEAR 1865. Boston. 1866. J. H. Eastburn's Press.

A neat report of 59 pages, showing an expenditure of $178,-854.64, with an income of $197,000.91, giving an excess of income of $18,146.27. Such an hospital should be organized in St. Louis, and occupy a central position in the city. The fact demands the attention of our city fathers.

PHYSICIANS' REPORT OF ST. VINCENT'S INSTITUTION FOR THE INSANE, FOR THE YEARS 1864 AND 1865. By J. K. BAUDUY, M. D.

The number of patients treated during the year was 109 males, 144 females; total, 253.

Married,	23	males,	43	females,	total,	66.
Single,	42	"	27	"	"	69.
Widows,	—	"	8	"	"	8.
Widowers,	5	"	—	"	"	5.
Restored,	31	"	25	"	"	56.
Improved,	27	"	36	"	"	63.
Unimproved	7	"	6	"	"	13.
Died	5	"	2	"	"	7.

The report is well arranged, and full of judicious remarks regarding the several forms of disease, and the best mode of treatment.

ESSAY ON THE PYROPHOSPHATE OF IRON. By E. N. CHAPMAN, M. D., Professor of Therapeutics and Materia Medica, Professor of Clinical Obstetrics, and Physician in the Long Island College Hospital. With notices of its Combinations and Forms of Preparations. Caswell, Mach & Co., 5th Ave., N. Y.

SUCCESSFUL REMOVAL OF THE UTERUS AND BOTH OVARIES, BY ABDOMINAL SECTION; the tumor, fibro-cystic, weighing thirty-seven pounds. By HORATIO R. STORER, M. D., Boston.

ANNUAL REPORT OF THE SURGEON GENERAL U. S. ARMY, 1865.

THE APPLICATION OF SUTURES TO BONE, IN RECENT GUN-SHOT FRACTURES, WITH CASES; also, Remarks on their similar use in some other Fractures and Operations. By BENJAMIN HOWARD, Esq., F. R. C. S., Hon. Sect., London. 1865.

BRAITHWAITE'S RETROSPECT FOR JANUARY. Part LII. 1866. New York: W. A. Townsend, 55 Walker Street.

In the next July number of the *Retrospect*, an American Appendix will be added, entitled "Half-Yearly Digest of the Medical Sciences:" comprising a practical summary of every discovery and improvement in medicine and surgery in the United States, during the previous six months.

The subscription to "*Braithwaite*," including the American "*Digest*," $3.00; when not included, $2.50; the "*Digest*" separtealy, 80 cents a year. Single numbers, 50 cents.

☞The Reviews and Bibliographical Notices have been limited, as also the Editorial Department, for want of space and time.

It will be observed that the present number of the *Journal* is one month earlier than usual, and appears at the proper time. In order to accomplish this the Observations of Drs. Engelmann and Wislizenus in the Scientific Department have been made only for March. Their valuable tables and observations will hereafter include two months, as formerly, commencing with April and May; also, the reviews and editorials will be more full.

Our object in the arrangement of the *Journal* is to afford our readers the most important matter, as Original Papers, Reports of St. Louis Medical Society, Review of Current Literature, etc.

☞Our thanks to Dr. C. L. French of Chico, California, who has favored us with a box of beautiful specimens of minerals, sholls, Indian skull, etc., which we will deposit for reference wtih the Academy of Science.

RECEIPTS NOT ACKNOWLEDGED BY LETTER OR OTHERWISE.

Drs. Henry W. Williams; J. N. Castlio, Hamburg, Mo.; C. S. Hertich, St. Genevieve, Mo.; W. A. Brock, Camden Point, Mo.: A. P. Spencer, Rocheport, Mo.; N. M. Smith, Kingston, Mo.; E. E. Webster, Carlinville, Ills.; F. S. Bodle, Owatonna, Minn.; T. A. Kingston, Fidelity, Ills.; T. L. Dean, Marion, Ky.; Woodson Morton, Burlington, Kansas; C. Q. Chandler, Rocheport, Mo.; J. W. Rose, Spring Hill, Mo.; J. M. Kennedy, Elk Grove, Mo.; J. W. Stewart, Clinton, Mo.; Salmons & Tucker, Louisville, Mo.; H. B. Brandt, Brandt's Rock Spring, Mo.; H. R. Allen, Charleston, Ills.; F. M. Scrogins, Glasgow, Mo.; J. T. R. Wadsworth, Collinsville, Ills.; W. W. Macfarlane, Concord, Mo.; J. R. Talley, Jackson, Mo.; W. C. Talley, Jackson, Mo.; S. P. Johnson, Oswego, N. Y.; W. N. Macfarlane, Concord, Mo.; T. J. Wright, Chilhowee, Mo.; D. L. S. Bland, St. Louis, Mo.; D. D. Emons, Pilot Knob, Mo.; L. J. Jones, Liberty, Mo.; J. D. McFadden, Carrollton, Mo.; N. L. Hornsby, Carondelet, Mo.; A. Parkhurst, St. Louis, Mo.; W. J. Heddens, St. Joseph, Mo.; Thos. Finley, Pana, Ills.; A. W. Milster, Attenburgh, Mo.; P. E. Wood, California, Mo.; T. W. Entrikin, Findlay, Ohio; J. F. Kerfoot, Trenton, Mo.; R. Scheffman, St. Louis, Mo.; J. H. Peabody, Omaha, Neb.; C. L. Young, Central P. O., Mo.; Geo. Hopkins, New York City; Arthur Hazlewood, Memphis, Tenn.; H. Frasse, Savannah, Mo.; Thos. Peevy, Wellsville, Mo.; J. W. Crawford, Carrsville P. O., Ky.; W. C. Overstreet, Smith City, Mo.; N. H. Martin, Princeton, Mo.; J. A. Pendowis, Jamestown, Mo.; F. W. Wesseler, St. Louis; F. O. Reilly, St. Louis; — Luyties, St. Louis; J. B. Sparks, Harrodsburgh, Ky.; J. R. Washington, St. Louis; Dan. Kuhn, St. Louis; A. Wiley, Ottawa, Kan.; Alfred Head, Millersburg, Mo.; G. E. Frazier, Pigeon Creek, Mo.; S. H. Grooms, Elkton, Ky.; J. E. Henry, Frirmount, T. P. McHenry, Dresden, Mo.; J. A. C. Brown, Dresden, Mo.; H. E. Depp, Sedalia, Mo.; H. Wichman, for '65-'66, Rock Island; Ruffin P. Small, Lexington, Mo.; A. Hueckelhan, St. Louis; W. B. Oulten, St. Louis; Thos. Peevy, Wellswille, Mo.; F. Carsfon, New Offensburg, Mo.; W. C. Overstreit, Smith City, Mo.; W. S. Dyer, Tunnel Station, Mo.; Geo. S. Bryant, S. Louis, Mo.; R. Scheffman, St. Louis, Mo.; H. Fraser, Savannah, Mo.; L. H. Baker, Quincy, Ills.; H. B. Dwelle, City; S. R. Anderson, Granada, Kan.; G. W. Ira, Decatur, Nebraska; F. Staples, Winona, Minn. — Graves, Brunswick, Mo.; S. P. Ives, St. Louis, Mo.; W. H. Clutter, Noble, Ills.; — Jaminet, St. Louis; F. Ronalds, Graysville, Ills.; Crowse P. Greely, Mt. Carroll, Ills.; A. C. Bingham, Howard, Ills.; T. G. Comstock, St. Louis, Mo.; T. Hearring, Green Bay, Wis.; J. R. Asbury, Monticello, Mo.; H. Bloxum, Mt. Auburn, Ills.; Jas. L. Perryman, Belleville, Ills.; A. E. Simpson, Charleston, Mo.; J. L. Haw, Charleston, Mo.; J. M. Youngblood, St. Louis Co., Mo; N. Guhman, St. Louis, Mo.; L. L. Warner, St. Louis, Mo.; B. M. Griffith, Springfield, Ills.; — Kittle, Marshall, Iowa; J. M. McKim, Newark, Mo.; Jas. Middleton, Carondelet, Mo.; W. C. Baird, Mandeville, Mo.; Geo. W. Weems, St. Joseph, La.; J. P. H. Gray, California, Mo.; J. H. Bradford, Council Grove, Kan.; D. D. Wood, Arrow Rock, Mo.; H. G. Gibson, Boonville, Mo.; J. Leigh, Highland,

Kan.; Geo. Coil, New Florence, Mo.; R. C. Prewitt, Louisville, Mo.; J. S. Van Hoy, Harrisonville, Mo.; V. H. Harrison, Clarkton, Mo.; R. M. Fowler, Paulville, Mo.; C. G. Thalman, St. Louis, Mo.; Wm. Anthony, Olathe, Kan.; S. J. Rhoads, Metropolis, Ills.; Black & Pond, Clayton, Ills.; David Prim, Jacksonville, Ills.; T. J. Means, Bardolph, Ills.; S. L. Berry, Cromwell, Ky.; O. F. Renick, Wellington, Mo.; W. C. P. Butman, Arnheim, Ohio; B. R. Senseny, Chambersburgh, Pa.; J. Hale, Owensboro', Ky.; G. Y. Hatcher, Carrolton, Ills.; H. V. Anler, St. Louis, Mo.; E. C. Royston, Huntingdale, Mo.; M. McCoy, Boonsville, Mo; Shook & Lomax, Laclede, Mo.; M. Bailey, Topeka, Kan.; W. M. Tuttle, Providence, Mo.; Thos. Kennard, St. Louis, Mo.; M. H. Woodfin, Syracuse, Mo.; C. H. Morton, Smithsville, Mo.; C. A. Smith, Galatia, Ills.; P. A. Rosenberger, Petersburg, Ills.; J. R. Hereford, Florisant, Mo.; T. W. Horton, Stiles, Iowa; S. C. Thomas, Waverly, Mo.; H. W. Jones, Pevely, Mo.; S. J. Harrison, Osage, Mo.

MORTUARY STATISTICS.

Abstract of Deaths occurring in the City of St. Louis, for the four weeks ending March 30th, 1866.

WEEK ENDING	SEX.	AGES. Under 5 Years.	5 to 10.	10 to 15.	15 to 20	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	Total.
March 9.....	Males.....	22	2	1	2	5	3	5	3	2	2	1	..	48
	Females....	16	1	1	..	2	4	1	2	1	..	..	..	28
" 16.....	Males.....	19	4	1	1	5	4	1	4	1	1	..	..	41
	Females....	14	..	..	1	3	6	1	1	..	..	..	..	26
" 23.....	Males.....	13	2	..	2	6	4	1	5	..	3	..	1	37
	Females....	9	2	..	1	2	2	..	..	..	..	1	..	17
" 30.....	Males.....	17	1	3	1	8	2	6	4	1	1	..	..	44
	Females....	15	2	..	2	2	3	1	4	..	2	..	1	33
Total.....	.....	125	14	6	10	33	28	16	23	5	9	2	2	273

There were also 31 Infants Still Born.

Statement Showing the Diseases resulting in Deaths, for the Four Weeks ending March 30th, 1866.

Angina Pectoris, 1; Aneurism of Heart, 1; Abscess Abdominalis, 1; Accidents, 5; Angina Membranosa, 1; Ascites, 3; Atrophia, 1; Apoplexy, 2; Asthma, 1; Bronchitis, 4; Bilious Diarrhœa, 2; Burn, 1; Cerebritis, 1; Convulsions, 14; Cramps, 7; Consumption, 19; Congestion, 2; Croup, 8; Congestion of Lungs, 6; Congestion of Brain, 5; Concussion of Brain, 1; Cancer, 4; Chronic Diarrhœa, 6; Cranio Tabes, 1; Congestive Fever, 1; Constipation of Bowels, 1; Diarrhœa, 5; Debility, 10; Disease of Bowels, 1; Disease of Heart, 1; Disease of Liver, 2; Delirium Tremens, 1; Drowned, 1; Dyspepsia, 1; Eclampsia, 2; Enteritis, 3; Erysipelas, 2; Fever, Typhoid, 15; Fever, Nervous, 2; Fever, 1; Fever, Puerperal, 2; Fever, Lung, 1; Gangrene, 2; Gun-shot Wound, 2; Hepatitis, 2; Hydrocephalus, 7; Hœmorrhage, 1; Hydrops, 2; Hypertrophy Cardis, 1; Inflammation of Brain, 4; Induration of Liver, 1; Intro-susception, 1; Lumps in Lung, 1; Lockjaw, 3; Meningitis, 6; Malformation, 1; Marasmus, 1; Moribund, 2; Neuralgia of Bowels, 1; Necrosis, 1; Old Age, 1; Phthisis, 20; Pneumonia, 35; Paralysis of Spinal Cord, 1; Peritonitis, 1; Pulmonary Abscess, 1; Pulmonary Congestion, 1; Still Born, 31; Spasms, 9; Scarlet Fever, 3; Small Pox, 3; Trismus, 2; Teething, 1; Tabes Mesenterica, 2; Ulceration of Bowels, 1; Unknown, 4.

THE SAINT LOUIS

Medical and Surgical Journal.

JULY AND AUGUST, 1866.

Original Communications.

BIOGRAPHICAL SKETCH OF JOHN BARNES, M. D.

Dr. John Barnes, the subject of this memoir, was born in the city of Philadelphia, and State of Pennsylvania, on the 2d day of December, 1791, being now in his seventy-fifth year. At the close of his collegiate education in the college, he entered as a private student of medicine, the office of Dr. Benjamin Smith Barton, Professor of Materia Medica, Natural History, and Botany, in the University of Pennsylvania, in which situation he continued until the spring of 1812. Having attended three courses of lectures, under those distinguished luminaries of the profession,—Doctors Rush, Physick, Wistar, Barton, Coxe, and Dorsey, whose names honor the pages of professional literature, he was graduated a Doctor of Medicine.

In 1813, Dr. Barnes delivered the annual oration before the Philadelphia Medical Society, on which occasion the Society tendered him a vote of thanks, and requested a copy of the oration for publication.

Dr. Barnes was the first *elected* member of the Academy of Natural Sciences, of Philadelphia, which was established in the spring of 1812; and soon after his election was appointed Recording Secretary, which official station he retained, by repeated election, for several years.

In the month of June, 1812, the United States Congress declared war against Great Britain; and immediately after that declaration, Dr. Barnes received the appointment of Surgeon of Col. Shinn's regiment of Pennsylvania Militia, which situation he held until the close of the war, in 1815.

In 1814, he was elected one of the Corresponding Secretaries of the Philadelphia Medical Society, which situation he held for several years.

The Academy of Natural Sciences, of Philadelphia, deeming it expedient to encourage a taste for the study of the Natural Sciences, and also with the design of promoting the popularity of their own institution, established a popular course of lectures on Botany. Doctors Barnes and Waterhouse were selected for the performance of that important duty, which they executed by the delivery of two courses of highly popular lectures.

In 1817, Dr. Barnes published, in Chapman's Journal, an important practical paper on "Gangrenous Suppuration," which received the highest attention in every part of the country. Dr. Eberle having become the editor of the Philadelphia Medical Recorder, Dr. Barnes took an active part in the literary labors of that journal, writing many of the editorial notices, and contributing many important original papers.

In 1821, he was elected, by the City Council of Philadelphia, a member of the Board of Health, and re-elected in 1823.

In 1826, he was made Professor of Midwifery, in Jefferson Medical College, and delivered two courses of lectures in that institution.

In 1828, at the request of Professor Jackson, he published in the Philadelphia Medical Journal a full Analytical Review of "Louis on Typhoid Fever." This review embraces over forty pages of that journal; and as there was no English translation

of that important work until several years subsequently, the English student was obliged to resort to this Review for a knowledge of the work of this great French Pathologist on this important disease.

In 1830, Dr. Barnes removed to the State of Mississippi, and was soon after appointed one of the medical Censors of that State, whence he afterward removed to Louisiana; and after an extensive practice for twenty years in the States of Mississippi and Louisiana, he removed to the State of Missouri in 1841, establishing himself in the city of St. Louis. Here he took an active part in the medical and literary institutions of the city, and on the visitations of the cholera in '49, '50, and '51, he was appointed, with Dr. Benkendorf, to the charge of the Quarantine Hospital, and as Quarantine Physician.

In 1848, Dr. Barnes contributed a number of papers to the St. Louis Medical and Surgical Journal, some of which were republished in the European medical journals.

We notice among these valuable papers the following:—

1. An article on the Rubefacient and Counter Irritant Effects of Potassa fusa in certain forms of Disease.

2. On the Medical Profession Proper, in Relation to its Various Fractional Parts of Homeopathy, Hydropathy, and Botanic Medicine.

3. A very severe Review of the Prospectus of the Eclectic Institute of Cincinnati.

4. Some Reflections on the Severity of Human Parturition, as contrasted with that of Brute Animals.

5. A Review of Bischoff on Generation.

6. On the Variola Nigræ, or Black Small Pox.

On the death of Professor Chapman, of Philadelphia, Dr. Barnes, at that time President of the St. Louis Medical Society, was selected to deliver the eulogium on this distinguished physician, which address was published in this Journal at the time.

In 1853, Dr. Barnes was elected Professor of Materia Medica, Therapeutics, and Medical Botany, in the Missouri Medical College, which situation he retained until that institution was sus-

pended by the late civil war, since which time he has been in the private practice of his profession. During his connection with this institution, he delivered two valedictory addresses to the students, both of which were published at the request of each of the graduating classes.

Dr. BARNES is still in our midst, an old and honored member of the profession, distinguished for his many mental, moral, and social excellences, which command for him a high and worthy position among his brethren.

His urbane manners, and distinguishing marks, as a gentleman; his extensive experience in practice, and familiarity with the medical literature of his country and times; and his remarkable conversational ability, endear him to every member of the profession of St. Louis, and will hand his name down in the pages of its history, as a cherished member and exponent of the times through which his career as a physician extended. He may be appropriately called a great and good man; and his foot-steps have led him to the rich awards of the *happy* old age he has so justly attained.

Dr. BARNES has two sons practicing medicine and surgery, who have already gained an enviable place among the more successful practitioners of our city.

We have been able to make but a brief sketch of Dr. Barnes, whose life and works should be preserved in a more complete form than can be given in the pages of a Medical Journal. Having the buoyancy of youth and an activity of mind seldom retained so perfectly at his age, we may yet look to him for its accomplishment in a full account of his life, and writings, in book form.

REMOVAL OF THE UTERUS AND BOTH OVARIES BY ABDOMINAL SECTION—DEATH FROM SHOCK IN THREE HOURS.

BY PROF. CHARLES A. POPE, M. D.

Some months ago, previous to the publication of his very interesting and valuable pamphlet on the "Successful Removal of the Uterus and both Ovaries by Abdominal Section," I received a letter from Prof. H. R. Storer, of Boston, inquiring whether I had ever performed such an operation, or knew of any other than those published by Clay, Kimball, Burnham, Koberle, *et al.* In my reply in the negative I requested him to furnish me with the particulars of his case, as I feared that I had a somewhat similar one in my own practice, for which I might soon be called on to operate. This fear proved but too well founded. On the 12th of April, I removed the womb, with both ovaries, by abdominal section; my patient dying, unfortunately, from shock, three hours subsequently.

The history of the case is as follows: Mrs. L., a lady 47 years old, the mother of three children, of whom the youngest is now fifteen, although of spare habit, has generally enjoyed tolerably good health. More than twenty-five years ago, and but a month after one of her confinements, she walked two miles, on a very hot day, to visit some friends. Since then, she has always complained of more or less uterine trouble. About twenty years ago, she observed a tumor in the left iliac region. It was as large as a hen's egg, and gradually increased in size. In October, 1864, she noticed another tumor on the right side, which enlarged more rapidly, and soon surpassed in growth that of the opposite region. I first saw Mrs. L. five months ago. Her health was feeble, her countenance chloro-anæmic. She complained of much discomfort on account of the abdominal enlarge-

ment. This consisted of an irregular tumor or tumors filling three-fourths of the abdominal cavity, more marked on the right than on the left side, having a sulcus, or depression, at the linea alba, extending upwardly to the left, and corresponding to the interval formed by the uterus between the lateral prominences. The uterus, *per vaginam*, seemed enlarged, irregular, and immovable. Large, rounded swellings, more considerable on the right than on the left, were felt on both sides of the womb, which swellings were formed by the pelvic prolongation of the abdominal tumors.

Having learned from others that there was no relief but from an operation, the patient was desirous of submitting to it at once. I candidly told her of its danger, which she seemed unwilling to admit. Allowing her time for consideration, I informed her that if, on reflection, she still wished the operation, I would yield to her request, and perform it.

For four months subsequently, I saw nothing more of the lady. At the end of this time, her husband desired me to visit her. He stated, that for four months his wife had insisted on seeing me, but he had put her off by saying that I had advised a further postponement of the operation. On discovering the deception, she determined to visit me herself. She was saved this trouble by my being called to revisit her. I now found her rather less reduced in flesh and strength than I had expected. She was suffering from nausea and vomiting, caused by pressure of the tumor, and her feet and legs were greatly swollen by œdematous effusion. She instantly demanded to be relieved, if possible, by an operation, saying that if refused, she would, with a razor, attempt it herself. At my request, Dr. M. M. Pallen carefully examined the patient, on two occasions of five months' interval, and each time gave, as his opinion, that although a bad case, an operation was yet proper and justifiable. The patient, with that courage which in such cases enlists the sympathy and excites the wonder of the surgeon, herself named the day, the 12th of April. Upon this determination, she seemed relieved in mind and hopeful of the result.

The usua preparations having been made, and the patient put under the influence of chloroform, assisted by Drs. M. M. and M. A. Pallen, Gregory, and Anderson, I made an incision along the linea alba, from the umbilicus to the pubes. A large vein having been opened, and bleeding freely, I applied ligatures to the cut extremities. The peritoneum was reached and divided, so that I now passed in my hand and explored the tumors. There were some superficial adhesions which were easily overcome, but I found much resistance posteriorly. The uterus was readily recognized as forming the connection between the twin tumors constituted by the enlarged and diseased ovaria. Finding the tumor much too large to pass through the external incision, with a large trocar and canula, I tapped the right sac, and drew off a quantity of thin, puriform liquid; but, owing to the clogging of the tube, I could not empty the sac. I therefore made, with the bistoury, an incision sufficiently free to permit the passage of numerous large flocculi and fibrinoid concretions. Tapping was also performed on the left sac, the liquid from which was thick and purulent, and emitted a heavy, fetid, and sickening odor. The general mass, although now much reduced, could not yet be brought out through the external opening; so that the incision of the linea alba was prolonged for two and a half inches above the umbilicus. In the depression between the uterus and left ovary, the small intestine was firmly adherent, as was also the sigmoid flexure of the colon, quite extensively. By occasional touches of the knife, but mainly by the finger-nails and tearing, I succeeded in detaching them. Some oozing hemorrhage followed, but not considerable, and it was soon arrested. But the worst adhesions were posteriorly and below. These I overcame as far as I could; but the tumors dipping deep into the pelvis, and the patient becoming very feeble, I deemed it best to remove what I could, and with the large mass out of the way, to detach, if possible, any remaining portions of the cysts. With some difficulty, I included within the long chain of an écraseur, with a curved extremity—which admitted of a lower embrace of the parts than was possible with a straight instrument—the thick mass of tissues,

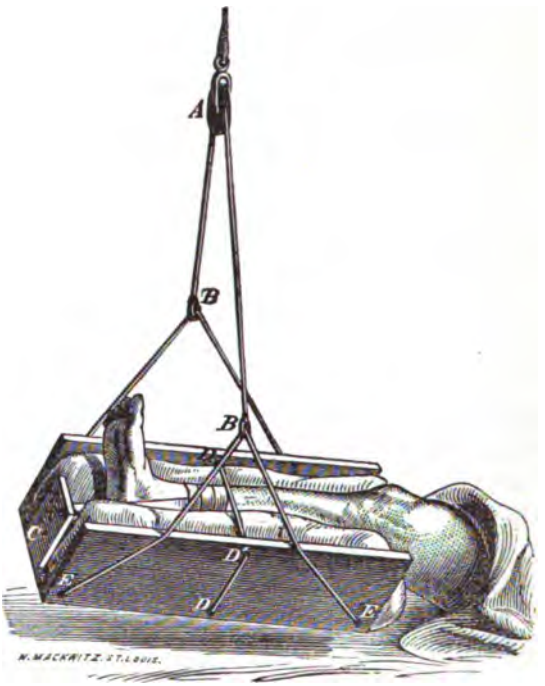
whose neck (for there was no pedicle) was nearly of the diameter of the pelvis. As this chain was forced down into the pelvic cavity and tightened, I drew up through its loop all that I could of these lowest portions of the sacs; but I found it impossible to include them entirely, a small part of each being left behind. The chain was made to divide the tissues within its grasp very slowly; and such was their resistance and quantity, that the strength of the instrument was taxed almost to its maximum. There was no hemorrhage. Clearing out the abdominal and pelvic cavities, and detaching what I could of the small remnants of the sacs, one of which was firmly adherent to the bladder, I replaced the intestines, which had protruded and caused some inconvenience during the operation, and then brought the external wound well and accurately together by wire sutures. A broad body bandage was placed about the abdomen, and the patient was lifted to her bed. She soon recovered from the effects of the chloroform, and seemed to bear the formidable operation as well as the most of my ovariotomized patients. I ordered some brandy toddy, with forty drops of laudanum. Although very feeble, she conversed with her husband, and seemed still hopeful. I, however, informed Mr. L. that I had not the slightest expectation of her recovery, and that she might die very soon. No reaction occurred, notwithstanding stimulants and external warmth, and the patient continuing to sink, ceased to breathe in three hours after the operation. No post-mortem examination was allowed.

On examining the removed mass, I found the whole uterus, with the exception of a very small portion of the neck. Its walls were greatly thickened, and occupied by numerous fibroid tumors, varying in size from a pea to a large walnut. One of these about the fundus, and as large as a pullet's egg, was almost entirely calcified. The uterine cavity was but little changed, and was occupied by a small flat mucous polypus attached to the fundus. The two sacs were closely adherent to the sides of the uterus, their walls being a quarter of an inch thick, and the lining membrane presenting the appearance of a mucoid, or rather of a pyogenic tissue, particularly that of the left side. The whole

mass,—tumor, sacs, and contents, would, I suppose, weigh from twenty to twenty-five pounds.

In reviewing the case, I must say, that had I foreseen its exact nature and difficulties, I certainly would not have operated. But, how impossible is it, even for the most experienced, to foretell all the complications of such and similar cases. That my impression as to its character was correct, may be seen by my letter to Dr. Storer. This gentleman, in his late admirable article in the January number of the American Journal of the Medical Sciences, has, I think, shown that the removal of the uterus and ovaries is a justifiable operation. He gives a table of all the instances in which it has been performed. They are twenty-four in number. The first was done by Mr. Clay, of Manchester, England, in 1843, which was fatal; the last of the twenty-four cases, by Dr. H. R. Storer, in 1865, which was successful. Of the first seventeen operations, two only, the fifth and sixth, were successful, the mortality being eighty-eight per cent.; whilst of the last seven cases, three only were fatal, and four successful, making fifty-seven per cent. of recoveries. Taking the whole number, there were six recoveries out of twenty-four operations, one in four, or twenty-five per cent.,—a rate which compares favorably with other major operations in surgery, such as the ligature of great arteries, the larger amputations, and ovariotomy. I regret that this last and unfortunate case has increased, rather than diminished, the per centage of mortality.

DR. HODGEN'S APPARATUS FOR SUSPENSION OF INFERIOR EXTREMITIES, IN CASE OF FRACTURES OR OTHER INJURIES.



The Splint, as exhibited in the wood-cut, is composed of three boards, each two feet long. The side pieces, which are fastened together at lower edge by means of cords, are eight inches wide; while the bottom piece, which should be light, is only six inches, and has attached to one end an upright piece, represented at *C*, for extension of limb. In order to suspend the box, two cords, of equal length, one upon each side, e. g., *EBE* should pass over pulleys at *BB*, which are fastened to ends of cord *BAB*, which passes over third pulley at *A*, and this is fastened to ceiling, or some point above, by means of a cord.

The great advantage of the Splint is in the rotatory movement permitted by pulley at *A*, which gives the patient a freedom of motion which is denied by the common splints. The pulleys at *BB*, permit flexion and extension of leg. The pulley at *A* should be large; the cord *BAB*, should be about four feet long; and the cord *EBE*, as also its fellow, should be fastened to lower edge of side pieces, to give easy and regular movement to the limb.

The only dressing that the limb needs, consists of two or three strips of adhesive plaster. Having completed the arrangements for suspension of the splint, we place a wide and thick cushion on the bottom of the box, and then place the leg in the box, bring the side pieces to a perpendicular position, and maintain them by means of a cord which passes around the box, or as represented by cord *DDD*. When the apparatus is well-adjusted, and the leg placed well up toward upper edge of box, the patient can turn from one side to the other with ease, thus obviating all danger of bed sores.

In compound fractures, the cord to which the pulley *A* is attached, should pass over a pulley in the ceiling, and have its end fastened to one end of a slide. This slide should be of hard wood, four inches in length by one in thickness, and have a hole in each end large enough to let the cord pass through. The cord ascending from *A*, passes through one end, and to the other, the end of cord is secured. By moving slide, the box can be elevated or depressed, at pleasure. When the box is let down, the cord *DDD*, and the fastening at *C* loosened, the leg may be lifted from its bed, and clean dressings applied.

The wood-cut was made from sketch, taken from the apparatus, as applied under direction of Dr. Hodgen, during last February, to a patient in the City Hospital having compound fracture of leg, and represents the splint as it appeared when he was lying on his left side. The apparatus gives to the patient a freedom of motion, which is not only a great comfort, but also a great benefit, and is not detrimental to the fracture.

H. H. M.

STRANGULATED HERNIA—OPERATION, AND PROBABLE RADICAL CURE.

BY F. RICE WAGGONER, M. D., PUEBLO, COLORADO.

Was called to see Mr. S., aged 55; a man tall, lean, and of nervous temperament, much debilitated from hardship and exposure incident to a frontier life. Had been the subject of an oblique inguinal hernia for thirty years, though he had controlled it very successfully with a truss, and but a few times required the assistance of a surgeon to reduce it. At about 6 o'clock of the morning above, he made an effort, thoughtlessly, and unguarded by his old companion, the truss, to shoulder a log of wood; the viscus descended, giving him usual pain; he dismissed his burden, returned to his room, reclined on his bed, and commenced the operation of taxis, so often practiced on himself on former occasions. But all manipulations of his failed. He called his friends to his assistance; repeated manipulations were persevered in until about 8 o'clock A. M., when the parts became so much swollen and painful, that they had to desist. All the symptoms of strangulation becoming manifest at that time, he at once ordered a messenger for medical assistance—a distance of twenty miles. We received his message, learned the probable nature of the case, and consequently repaired to his ranche, fully provided with all necessary instruments for an operation.

We reached the residence at 6 P. M., made an examination, and found our fears fully realized—a perfect strangulation. After making a few slight efforts to reduce it, without any promise of success, we informed our patient that an operation would be necessary for its relief. Consent was at once obtained, and all necessary arrangements were at once made for an operation. As darkness was fast approaching, and as the operation would necessarily be performed by the dim light of a taper or two, without any medical or other intelligent assistant, we confess to no small degree of embarrassment. The parts were tensely swollen, of a dark, glossy hue, indicating much inflammation, and probable change in the structures beneath.

No chloroform was administered, partly for want of assistance, and partly fearing the capacity of his lungs; but the services of two stout men were procured to control his limbs. The hernia was on left side. Position was taken, and first incision made — a bold one — of five inches long, through the integument and superficial fascia; dissection followed through the several structures; but, owing to the state of inflammation, and consequent change in the appearance of the anatomy, they were very hard of recognition; consequently, we were as much, anatomically, in the dark as we were from the flickering of tapers around us. The sac was eventually reached, opened, and some two or three ounces of fluid escaped. We found a segment of the ilium, with the major portion of the omentum. The hernia was of the oblique protruding variety; but from its long standing, the opening was drawn down, almost converting it, in appearance, into direct hernia.

The stricture, which was very tense, ligating the parts as perfectly as if it had been done with a whip cord, was divided. The gut was readily returned, but the omentum proved more troublesome; in fact, we failed in reducing the whole, leaving a portion almost as large as an orange in the opening, or outside of it, which we were tempted to remove with the scalpel, but refrained. The integuments were united by three interrupted sutures. Strips and water dressing were applied, the parts healed kindly, and our patient recovered, with but very slight constitutional disturbance.

Up to present date (March 9), it has every appearance of a radical cure. We think we have plugged the opening; and adhesions having formed, a radical cure is probable. Another point of interest to the writer was in the ready restoration to a healthy condition, of the portion of the ilium strangulated after it had approached, to all appearances, nearly to gangrene, with but slight derangement of the functions of the bowels.

Correspondence.

A LETTER ON SYPHILIS.

NEW YORK, May 9, 1866,
162 W. 23D STREET.

DRS. LINTON & WHITE:

Gentlemen: I have been much interested in a series of articles appearing in your valuable Journal, by Dr. A. C. Haynes, entitled "Syphilitic Malady." The history of the two diseases, recently confounded under the name of "syphilis," is admirably given, and their distinction fully recognized. It appears to me unfortunate, however, that the two diseases are still included under the name of "Syphilitic Malady," while only one of them is syphilis; and, also, that the terms "soft" and "hard chancre" should be perpetuated; since, so long as the attention of the profession is exclusively directed to the initial lesions, just so long is the main ground of our belief in their distinct nature—a local character upon the one hand, and a constitutional character upon the other, perpetuated in successive generations—liable to be overlooked or doubted.

I commenced this note, however, with a different object in view than discussing this question.

Most of us who put "the finishing touches" (Heaven save the work) upon our medical education, by a sojourn in Paris fifteen or more years ago, left that city dyed in the faith that in the teachings of Ricord, in the wards and under the shade trees of the court-yard of the Hôpital du Midi, was to be found the most advanced expression of venereal pathology. Since then the world has been turning round, and some new views, apparently

worthy of attention, though somewhat at variance with the doctrines of our old teacher, have come up. Meanwhile, we have been anxiously looking for some expression of opinion from our "*magister*," to see if he confirmed the views which practice has taught us to be correct. But our desire has been only partially gratified.

Ricord's own latest work, "Lettres sur la Syphilis," has reappeared in several editions with only trivial additions and modifications. His "*Leçons sur le Chancre, redigées et publiées par A. Fournier*," are, indeed, later; but in reading them one hardly knows what to ascribe to the lecturer and what to the editor. Again, in the discussion before the Parisian Academy of Medicine, and elsewhere, the remarks of Ricord have too often been oracular and ambiguous to furnish a key to his decided opinions.

In this frame of mind, it was with peculiar satisfaction that we read the title of Dr. Haynes' first letter, concluding thus: "A few questions to M. Ricord, with his reply," &c., &c. As each letter has appeared, we have hurried through the pages before deliberate perusal, to find the fulfillment of the promise expressed in the title. At last comes "concluded," in which we find a section entitled, "Questions and answers." Are these questions to, and answers from, M. Ricord? The logical conclusion from the heading of the letter is in the affirmative, but nothing in the text indicates it, and a matter of this importance to some of your readers should have the official stamp of authority.

Allow me, through your columns, to thank Dr. Haynes for his valuable communications, and ask him to confirm the *fact*, as I take it to be, that the "Questions and Answers" were the substance of one or more conversations between himself and M. Ricord. Respectfully and truly yours,

F. J. BUMSTEAD.

A REMARKABLE CASE OF SUCCESSFUL GASTROTOMY.

MEMPHIS, TENN., March 18th, 1866.

DEAR DOCTOR: By to-day's mail I will send you the first number of the Medical and Surgical Monthly, in which you will find a report of a case of successful Gastrotomy.

Since the publication of this article, I chanced to see an old copy of the Philadelphia Medical Examiner, dated October, 1854, in which I find reported a somewhat similar case. The operation was performed by John Neill, of Philadelphia. The editor, in a note, states that "this is the first case of successful Gastrotomy in the United States."

Within the last two years, two successful operations have been performed in England ,and were reported in Braithwaite's Retrospect.

As a matter of history, it would be well to have the report of the case in this number of our Journal, republished in the St. Louis Medical and Surgical Journal. With this suggestion, I leave the matter in your hands.

As ever, your friend,

E. MILES WILLETT.

The following is the case of Rupture of the Uterus, in which Gastrotomy was successfully performed:

In the summer of 1859, Mrs. V., aged twenty-one, a native of Italy, was delivered with forceps, at full term, by Dr. Frayser and myself. The child was born dead, owing to the tedious labor and prolapsion of the umbilical cord. In 1860 she was again delivered with forceps of a lifeless child. The following year she returned to Italy, and was confined near Genoa. When the waters broke, an arm came down; the accoucher turned and delivered; but the child perished. After each labor, the convalesence was speedy, and without a single bad symptom.

On Sunday night, March 20th, 1865, she was again seized with labor, but as the pains were inconsiderable, the physician was not disturbed until four o'clock, A. M., when my partner, Dr.

Frayser, was summoned to her assistance. After making an examination, he did not deem it necessary to remain, and consequently returned home. At eight o'clock he visited her again, but perceived nothing remarkable in the case; the os uteri was soft and dilated, while the child's head seemed about to engage in the superior strait; but the pains were neither frequent nor hard, and there was but little sympathetic disturbance of the system.

At nine o'clock, Dr. Frayser told me that Mrs. V. was in labor, and as he anticipated difficulty, very kindly requested me to see the case with him.

We found the patient somewhat restless and disturbed in mind. She had vomited freely, and was throwing up bile when we entered the room, but had not had a labor pain for half an hour. The blood in the vagina, the recession of the head, the absence of labor pains, and the irregular surface of the abdomen, through the walls of which could be distinctly felt an elbow, made the diagnosis of Rupture of the Uterus absolutely certain; although the attendants did not think that she had suffered much, and could not remember any particular agonizing pain.

After consultation, we decided to perform Gastrotomy. The sudden announcement of the exceedingly dangerous condition of the patient, and the explanation of the proposed formidable operation, produced such an effect on the feelings of her husband and friends as can *only* be imagined. That they should have withheld their consent, for awhile, to the performance of an operation which offered so little prospect of success, is not surprising. In the midst of this delay, the patient herself requested us to do whatever we deemed best. At twelve o'clock, therefore, two hours and a half after the rupture occurred, I commenced the operation, assisted by Drs. Frayser and Grant, by making an incision in the median line, through the skin and cellular tissue, from below the umbilicus to within an inch of the pubes; but as this would not give sufficient room, I extended it upward and to the left of the umbilicus. I then opened the peritoneum, and with a probe-pointed bistoury, guided by my finger as a director,

completed the incision. We found that the child and placenta had been expelled by the uterus into the cavity of the abdomen, the child's head rested on the brim of the pelvis, and the uterus had contracted sufficiently to prevent exhausting hemorrhage. After removing them, we sponged out the blood and amniotic fluid as well as we could, drew the edges of the wound together with interrupted sutures and adhesive plaster, adjusted the body bandage, and ordered cold applications. During this time the patient was fully under the influence of chloroform. The pulse was ninety-six, and sufficiently full before the operation; one hour afterward it increased to one hundred and ten. During the night she took six drachms of elixir of opium.

Tuesday, 9 *A. M.*—Pulse one hundred and ten, and full; skin hot; abdomen tympanitic, but without tenderness; passed water during the night, and rested tolerably well. Ordered six drops of veratrum viride to be taken every three hours; one grain of calomel every two hours; elixir of opium in sufficient doses to relieve pain; vaginal injections of tepid water, and cold applications over the abdomen.

Six P. M.—Pulse one hundred and twenty; tympanites very considerable; ordered mist. assafœtid. to be injected into the bowels.

Wednesday, 9 *A. M.*—Bowels moved twice; tympanites relieved; pulse ninety-six; skin moist; passed urine several times without inconvenience; slept well; lochia good. Discontinued the veratrum and calomel.

March 23d, 9 *A. M.*—Pulse eighty-four; skin moist; no tympanites; bowels moved once; vomited once during the night, but rested well.

Vespere.—Pulse eighty-four. Patient complains of pain in the abdomen, has troublesome nausea, and has vomited several times during the day. Ordered starch and laudanum injections, and turpentine to be applied over the region of the stomach.

Friday, March 24th, 9 *A. M.*—Pulse ninety-six; skin moist; passed water; bowels moved; vomited once, but rested well.

Vespere.—Pulse ninety-six; vomited bile; thirst increasing; tympanites considerable; patient more comfortable, with legs drawn up. Ordered enema of mutton tea.

March 25th, 9 *A. M.*—Pulse one hundred and eight; bowels moved frequently; vomited bile; nausea continues. Dressed the wound, which looks very well. Ordered lime water and milk, and injections of starch and laudanum.

March 26th, 9 *A. M.*—Pulse one hundred and ten, feeble; bowels moved twelve times since the last visit; vomits bile occasionally; no tympanites; skin moist; kidneys act well. Ordered animal broths, and brandy, and opiate injections.

Vespere.—Bowels moved frequently, sometimes with pain; vomiting continues, but at longer intervals. Patient complains of general distress.

March 27th, 7 *A. M.*—Pulse one hundred and eighteen, with sufficient strength; no tympanites; no tenderness; lochia slight. During the night the bowels were moved frequently, but only twice to-day. At present she is very comfortable, and quite cheerful. She has taken chicken water, and a small quantity of Château-Margaux. Ordered elixir of opium, *pro re nata.*

March 30th.—Pulse one hundred and eight; lochia dark, profuse, and exceedingly offensive; abdomen tympanitic, but without pain; skin natural. Changed adhesive straps and removed sutures. The wound was well united, except at the lower portion. Ordered four drops of nitro-muriatic acid to be taken every four hours, and vaginal injections of diluted permanganate of potash.

March 30th.—Pulse one hundred and eight; lochia unchanged; abdomen greatly distended. Patient is low spirited, and refuses to take the acid. We substituted bisulphite of soda, and ordered mist. assasfœtida to be injected into the bowels.

April 1st.—Pulse one hundred and eight, good strength; bowels moved once; distention of the abdomen reduced; passed water as usual; tongue looks well; rested comfortably.

April 3d.—Wound entirely united; pulse one hundred and eight; bowels in good condition; vaginal discharge considerable, and very offensive. Ordered diluted chlorinated soda to be

alternated with the permanganate, as an injection. From this date, the vaginal discharge gradually diminished, and became less offensive; and under the influence of iron, wine, and nutritious diet, the patient, in a few weeks, regained her former health, and strength, and beauty. In due course of time, the menstrual function was established, and since then has been regularly performed in a physiological manner.

Transactions of Societies.

PROCEEDINGS OF THE ST. LOUIS MEDICAL SOCIETY.

REPORTED BY R. S. ANDERSON, M. D., RECORDING SECRETARY.

[Continued.]

Dr. Hammer said, that the subject to which he had referred at the last meeting was that of *inflammation*. During a period of years the Society has discussed practical questions. If we examine the various treatises on Pathology, we will find at the bottom of all diseases called "peculiar diseases," a peculiar process called *inflammation*. This was a difficult question to discuss, and he knew that his views would meet with considerable opposition from some members of the Society. It was impossible, from the vastness of the subject, to give a general and complete discourse, and he would, therefore, confine himself principally to the points of dispute.

From the time of Celsus to that of Rokitansky, inflammation was indicated by its chief symptoms, "rubor, dolor, calor, icor." Rokitansky made a new classification, regarding inflammation as hyperæmia and exudation, regarding active and passive congestion as the same, and classifying the various diseases according to the different exudations. This innovation provoked much discussion, which finally proved the theory to be false, a fact which was acknowledged by the author himself.

What theory have we substituted for this? At the present time we define inflammation to be *irritation of a tissue, caused by an irritant agent, always followed by an efflux of nutrient fluid, with an increase of the formative process, and an increase of the tendency to degeneration of the same tissue.* This, of course, demands an explanation. Before this is entered into, we must go further back and inquire, what are the general vital processes?

1st. Nutrition, of which health is the perfection.

2d. The Regenerative Process, which is a variety of the Nutritive Process.

3d. The Progressive and Retrogressive Metamorphosis.

Under the head of *Progressive Metamorphosis*, we have:

1st. Hypertrophies, divided into—first, True; and, second, Numerical. A True Hypertrophy is an increase in the *elements* of a part. In the Numerical Hypertrophy, or Hyperplasia, the elements are not themselves increased, but there is an increase of the *number* of those elements.

2d. The Homeoplastic new formations. These are composed of growths, swellings, etc., the elements of which are similar to each other, and to the elements of the part in which they are situated. These are called Benign Tumors.

3d. The Heteroplastic Tumors,—those that do not possess the same elements as the tissues in which they originate. These are usually the Malignant Tumors. They are divided into—first, Heterostopic Tumors, those composed of the proper elements of some tissue of the body, but not those in the part in which they originate; and, second, the Hetero-Chronic Tumors—such as are

composed of elements which do not exist in the healthy state of the system in which they are found. These are *all* malignant.

Under the head of *Retrogressive Metamorphosis*, we have:

1. *Atrophy*, divided into—1st, True Atrophy; and, 2d, Numerical Atrophy. True Atrophy is that in which each element grows smaller; Numerical Atrophy is that in which there is a decrease in the *number* of the elements, they not being changed.

2. Under Retrogressive Metamorphosis are the *Degenerations*. 1st, Degeneration with preservation of the elements, they losing their function; and, 2d, *Necrobiosis*, a degeneration in which the elements are entirely destroyed and may not be recognized. Under the first variety of degeneration, in which the elements preserve their form, are—1st, Horny Degeneration; 2d, Induration; 3d, Tubercular; 4th, Calcarious; 5th, Amyloid. Under Necrobiopic Degeneration are—1st, the opaque, granular, albuminous, or fibrinous degeneration; 2d, the Hydropic Degeneration; 3d, Mucous; 4th, Colloid; 5th, Fatty; 6th, Melanoid.

These Hypertrophies and Atrophies may go on without any inflammation, inflammation being *always* characterized by *the presence of progressive and retrogressive metamorphosis.*

Any agent which is unusual in its action on the system is called an irritating agent, and such a contact will produce changes which deviate from the usual process of nutrition. Such an agent acting, causes an irritation of the tissue: the cells of the tissue are first attacked by the agent. It is not hyperæmia that produces the inflammation, nor exudation; but the genuine irritation takes place in the *cells* of the tissue. An inflammation may exist without either hyperæmia or fluxion. These changes may take place in organs having no vascular organization. To show this, we can take neither the muscular, fibrous, or osseous systems, but must select a tissue having no vascularity. The cornea is not adapted to the purpose, as only its central portion is absolutely non-vascular. The cartilages are more proper; they are non-

vascular, and get their nutrition from a distance. The cartilages of the joints receive their supply from two sources; the synovial membrane, which does not extend over them, and from the spongy part of the bones. Let us irritate a cartilage: a movement is seen in the cells, they swell and grow, and we see opaque granules filling them.

This is the degeneration spoken of above as the opaque, albuminous, or fibrinous degeneration. To accomplish this change the cells must have an afflux of nutrient fluid derived from ordinary sources. The degeneration continues in the cells; they split, break up; and all kinds of cell formation takes place. All the organized elements in an inflamed part are formed from the cells of the part. Next, as the increased afflux of fluid to the irritated tissue takes place, a more abundant flow of blood follows to the parts from which this is derived,—the synovial membrane, spongy bone, etc. Then we see the formation of new vessels; parts becoming vascular which were not so before. How are these formed? By an abundance of cellular elements accumulating in such order, as to leave channels in which the blood flows. Thus we have the processes of Progression and Retrogression always veiled. But we may have one or the other of these processes predominating, thus forming the different kinds of inflammation.

Hyperæmia, congestion, stasis are not acts of activity but of paralysis of the sympathetic nerve, as shown by Virchow. This, at least, is the most frequent cause. When we say that active congestion is not relevant to the process, we do not mean that it is not present, but that it is not a cause; on the contrary, it is a result of inflammation. In paralyzed limbs we often have congestion when none of the symptoms of inflammation exist; it is not a primary but a secondary symptom. It is the same with the exudation; which brings us to a remarkable point, that exudation is not necessary, and in some inflammations, as in the parenchymatous, it is absent. One of the striking features which have been quoted to overthrow this theory is that of fibrinous exudation, which is said to arise because of excessive

fibrin in the blood. But the fibrin in these inflamed parts is not derived from the blood, but is the consequence of the change in the cells, the debris of these being converted into fibrin by the true fibrinous degeneration. Fibrin is looked upon by most great pathologists as not a nutritive element, but as the result of waste. So, also, we may have an inflamed part become soft and elastic, what is called "plastic infiltration"; but this is also caused by the debris or the change going on in the tissue itself. You ask if in croup it is not possible to pull off the fibrinous exudation from the mucous membrane? But in this exudation you will find the debris of cells from the membrane. You do not find healthy tissue beneath, but diseased tissue, covered only by a new formation of epithelial cells. This shows that the great part of the membrane has been destroyed in the inflammation. Concerning Diphtheria, the difference between this and Croup has been denied by some authors. In some hand-books we find this distinction: If the inflammation is followed by an exudation, leaving healthy tissue beneath, it is croup; but if the infiltration of the fibrinous exudation grows deeper, so that we find the tissue beneath diseased, then it is diphtheria. If a severe irritation takes place, exceeding certain limits, the circulation in the irritated parts may be arrested, and if it is not restored by collateral circulation, then the tissue will die at once by necrobiosis; consequently, the metamorphosis can not go on, and we have a diphtheritic inflammation.

Dr. Hammer then passed on to the consideration of what have been called the cardinal symptoms of inflammation,—*heat*, *redness*, *pain*, *and swelling*, which have since been increased by the addition of *suspension or impairment of function*, *and fever*. None of these symptoms are sufficient to characterize inflammation, but it is the concurrence of these, or of a majority of them, that render them valuable.

First. We will consider redness, or, as it should be more properly called, *indirectly increased fluxion*. Now, this symptom alone can not characterize inflammation, as it may be produced at will, no inflammation being present. The redness of

inflamed parts makes it necessary to say something concerning hyperæmia. Books describe two varieties of hyperæmia. 1. Active hyperæmia or congestion; and, 2. Passive hyperæmia, or stasis. On what does active hyperæmia depend? On several different circumstances; as for instance, obstruction to vessels, as by pressure, thrombus, etc. From this an active hyperæmia ensues in the part, the action of the heart continuing to drive the same quantity of blood through a less number of vessels. The most common cause of active congestion is a momentary or lasting paralysis of the sympathetic nerve supplying the vessels of the part, as shown by the experiments of Claude Bernard, and others. What was called active congestion is therefore really paralysis. Active congestion always takes place in the arterial system. Passive congestion always takes place in the venous system, and generally arises from obstructions to the veins. It is of not so much importance as active congestion, as the system is much better supplied with veins than arteries, there being two systems of veins, the superficial and the deep. In this stasis the same amount of blood has to be carried through other channels by a lesser amount of vessels, so that instead of being a stasis, it is really an activity. Therefore, what was formerly called active congestion, is really paralytic; and what was passive, is, in reality, active, that is, if mere activity is taken into consideration.

We come now to the consideration of the action of the sympathetic nerve on the blood vessels. It has been proven by the experiments of Claude Bernard that the vessels are increased in size by cutting this nerve. Other interesting experiments have been performed by Ludwig, of Vienna. By irritating the lingual nerve, he found that the secretion of saliva by the parotid gland was much increased, it at the same time becoming thin and watery. Again, he irritated the sympathetic nerve supplying this gland, and at once the secretion was diminished, being thick and scant. He concluded that the sympathetic nerve was the motor nerve of the blood vessels, and that by irritating this the nutrient supply is cut off from those parts. The irritation of the lingual nerve producing contrary results, he concluded that these nerves

must exercise some influence on each other. He examined further, and found that by irritating the sympathetic nerve the blood became darker. By irritating the corda tympani, the blood became lighter or redder. If we irritate the sympathetic nerve, the number of the contractions of the heart is lessened; and if we cut or paralyze this nerve, the number and rapidity of the heart's actions is increased. We may therefore conclude, that if the sympathetic is cut or paralyzed, congestion is the result. Reasoning from these facts, we may conclude that there are other nerves which, being irritated, exercise a peculiar action on the sympathetic. Flayel has come to the conclusion that there are nerves belonging to the cerebro-spinal system, which exercise an inhibitory action on the sympathetic system. An irritation of a sympathetic nerve produces contraction, bringing on ischemia of the parts. Paralysis of a sympathetic nerve brings on hyperæmia. Irritation of a cerebro-spinal inhibitory nerve brings on hyperæmia. Paralysis of an inhibitory cerebro-spinal nerve produces ischemia. Returning to the symptom of redness, it is proven that this does not alone characterize an inflammation.

We will now consider the subject of Heat. It has been questioned if an inflamed part is really increased in temperature. We know that increased temperature does not always indicate inflammation; for the experiments of Claude Bernard have proven that hyperæmia alone is followed by increase of heat. But in inflamed parts there is really an increase of temperature, as shown by the ingenious experiments of John Simon, by means of an electric battery and needles inserted beneath the skin. This increase of temperature is not always the same, but depends upon the development of the inflammation. The development of heat depends upon the hyperæmia, and the more rapid molecular change in the tissue. Now we can see how proper it is to be careful in interpreting these symptoms.

As for the heat symptom—swelling—this will not suffice alone. We may have a tumor, either benign or malignant, with no inflammation, and we may have an inflammation with no swelling.

Pain alone is left; this may be produced by a variety of non-

inflammatory causes. In inflammation we have rather increased sensibility than pain. The pain may be explained in many ways. The inflammation may increase so as to exercise pressure on the nerves of the part; or an adhesive inflammation may cause adhesion and rupture of nerves, producing pain.

As to suspension or impairment of function, this may be real or apparent. It is real when such changes have gone on in the tissues as to render them unfit to perform their functions. But it is often only apparent, as when an inflamed organ is not used when its use involves so much pain as to render the patient unwilling to use it. However, sometimes other organs, not properly inflamed, may suffer in function on account of inflamed organs in their neighborhood. Suppose the glottis is inflamed; the swelling obstructs the air passages, and produces suffocation. All the elements of the larynx have their functions, but are interfered with by the inflammation of the neighboring parts.

There is one great symptom in inflammation left: If there is heat, redness, swelling, and pain, in the same part, we may reasonably conclude that there is inflammation; but if two or three of these are present and not all, we can not be certain; but if they are accompanied by fever, we may definitely conclude that there is inflammation. If a tissue be irritated, yet not to such a degree as to overcome the nutritive process, we have an inflammatory fever. On what does this depend? To a great extent on the reception into the system of waste matter, produced by the process of inflammation. These elements have to be removed by the circulation. They contaminate the blood, as they are materials not contained in normal blood. This brings on the symptoms of inflammation,—increase of temperature and pulse, loss of substance, &c. It was observed by John Simon that the blood in this condition contained more fibrin than healthy blood. Again, we have in the blood a change in the proportion of white and red corpuscles, the white being increased and the red diminished. This is only apparent, the disproportion being produced by the red corpuscles being broken down. We find also an increased number of lymph corpuscles. These facts led to

investigations which proved that the fibrin is not a nutrient material, but an excrementitous matter. By this also we can explain the fact, that in bleeding a patient in fever, we find what is called the buffy coat. The second bleeding shows more, and the third more still. From these facts, it is concluded that fever is a symptom of very great importance in inflammation. As far as treatment is concerned, as soon as we can open the secretions of the body, in the same ratio we can control the fever. By freeing the blood of these impurities by means of the kidneys, skin, etc., we arrest the fever. When a secreting surface or gland is inflamed, we do not, generally, have high fever; because the emunctories of the part are actively engaged in carrying off these foreign matters, and they do not thus contaminate the blood. This is, therefore, called Desquamitory Inflammation.

Dr. Watters said, Dr. Hammer, in his remarks, had given a résumé of the views presented in 1849 and earlier, the points of which were fresh in the minds of the members. Some of these views had been modified by later writers, whilst others are still adopted, although far from being explicit. For instance, when we say that inflammation consists in altered nutrition, it did not specify anything, as there is altered nutrition in all diseases. The vital question is, in what consists *that* altered nutrition which constitutes inflammation? In order to give some clue to the manner in which he desired to present the question to the minds of the members, he read certain passages from the London edition of Virchow's Cellular Pathology, in which was the following statement; namely, "Irritation must be taken as the starting-point of inflammation." Virchow, in this, does not differ from the most of writers on the subject. They all stop short of the real truth of the subject. The question would naturally arise, what is irritation? To judge from the manner in which it is used, it seems to be a boxed up category which a man can pull out when he wishes, and replace as suits his convenience or necessity. The true point to arrive at is to determine what is irritation: if we can arrive at this, we reach the foundation, and a point beyond that which served Virchow as his starting point.

The first proposition which he wished to bring before the notice of the members was, that every part of the body, every cell of which it is composed, lives for and by itself. Every cell is a distinct organism, and hence is that which determines its own mode of action. This is as true of the most complex organism—man—as it is of the most simple. The lowest orders of plants and animals consist in a mere accumulation of cells, one or several of which may be abstracted and placed under favorable circumstances, and the plant or animal will be reproduced, thus forming a new generation. The polyp may be cut into several pieces, yet each piece is able to reproduce a perfect animal. So it is with plants, as is most familiar with every one; a portion cut off and planted will produce a new plant. As this independence of cell and cell growth is true with the lower order of plants and animals, so it is true of man; the principle pervades every cell of which his body is composed. So far as he (Dr. W.) was capable of understanding Virchow, this principle was advocated by him, and it is this that distinguishes his work from all others published on pathology with which he was acquainted. But there is one point not recognized by him, except incidentally; and it is this that constitutes the defect of the book, as it is thus but a mere impression of an individuality. That point is, that while each cell lives by and for itself, it in that act lives by and for others. Had this truth been fully recognized, the "Cellular Pathology" never would have been written. The whole organism is a unit; man is a unit, though composed of an indefinite number of parts, each as independent as those of the protozoæ. Whilst the lowest of the animal series is merely a cell, in man the many cells are united so as to form a unit. Man is a negative unit—an organic unity—a unity in which each part is an integer. It is true that each part does its own work; yet each part in performing its own special duties, living by and for itself, at the same time lives for the whole. This is wherein the definite idea will modify the views of Virchow, as in the statement made by him that irritation is the beginning of inflammation.

Take any cell, whether from the highest or lowest organism: the cell itself is but one condition of life,—but one condition of the various processes, whether of growth, nutrition, or of function. Yet a cell is not capable of performing its actions except in connection with other conditions; namely, nutriment, or plasma, air and temperature. Life occurs in a simple, single cell among the lowest orders, but only when under the influence of these conditions. What is plasma? It is, itself, but the result of the action of other cells. In nutrition, reproduction, secretion, etc., there are influences under which the cell must be placed before these processes can be accomplished; yet there is no essential difference to be drawn between them; all are accomplished by the same general cell life. The human organism has certain objects to accomplish. It is sensible, thus differing from plants; and to attain this a certain modification of organization is necessary. Four certain conditions being essential in the production of life, we find there is in man specific provision for the attainment of them. We have a respiratory system, by which the atmosphere is carried to every cell in the organism; and each cell breathes the air just as truly as those of lower form do in the pools, and lives on the same principle, and in the same manner. Again: we have an alimentary system, as it is necessary to provide a plasma. The blood is the pool in which each cell lives, and an apparatus is provided by which the pool is carried to every portion of the body and bathes every cell. As man is subjected to the vicissitudes of temperature, special provision has been made so as to keep the body at a uniform height of temperature, and thus, with these modifications and provisions, we have the perfect whole; for in that temperature, and in that pool, and in that atmosphere, the cells, living for and by themselves, constitute the highest of animals,—man.

Every cell in living for and by itself, lives for and by the whole, and for others. Life is a process; it is not a certain state, but a movement, a change. It is immaterial whether we view this process as functional, nutritive, or developmental, as in the division by Virchow; the cell has a process peculiar to itself, and

this is its life. The cell in itself is inactive, and its activity, or the phenomenon of life, is due to other conditions,—plasma, or nutritive matter, air, and temperature. When the cell is properly organized and brought under the influence of the three conditions mentioned before, the movement resulting from this combination of conditions is the phenomenon of life. How is it that these conditions operate on the cell so that from an inactive it passes to an active state, and the process resulting is life instead of waste only?

In order properly to answer this interrogatory, he desired to go back and give the process of thought by which he was led to frame an answer. In 1850 or 1851, whilst studying Carpenter's Physiology (the edition of 1849), he found a statement by the author that life is a process; and in the discussion of the subject the question was asked, what is the condition of a seed in a granary? it is not dead, he says, for it undergoes no putrefaction; and yet it cannot be said to be alive, as it shows none of the phenomena of life. How is it explained? Only, as he asserted, that it is in a condition of dormant vitality, like the torpor affecting the frog in cold weather. This, in reality, is no explanation. He (Dr. W.) could not conceive how a force could exist and yet be dormant. That there is vitality in a seed is shown by its power of germinating, and yet how can a *force* become dormant? A dormant force is a self-contradiction. The idea then occurred to him that the force is not dormant, but just precisely as a spring, in recoiling, will produce certain motions according to the organization of the machinery to which it is attached; or, as a car running down hill will pull another up; or, as water falling from a height will exercise a power sufficient to move a mill. Just so in organic nature: a seed placed in the ground and the conditions of life applied, there results the same kind of change,—a recoiling, a destructive change,—and life is the reaction, the return. When we consider inorganic nature, action and reaction are equal, and there can be no process without this. All processes are the representatives of two opposite actions. Every element of matter has its properties, according to the conditions surrounding it, just

as the cell has. Hence, in chemistry, we have different substances formed from the same elements, according as the circumstances surrounding them are varied. Nitrogen and oxygen will unite in several proportions, and form substances differing from each other, just as the conditions surrounding them and the manner of presentation vary. An atom of oxygen removed beyond the earth's influence has no properties in itself. What it has is in and through the universe; *i. e.*, the conditions surrounding it. So the power produced by the recoiling of matter in the germination of a seed is the power represented by life: waste is the condition, and life the result.

The conditions of life are precisely the conditions of decay; where there is life, there is always decay,—a recoiling. We take into our bodies every day a certain amount of what is called nutriment, together with water and air; and the food, as strange as it may appear, is taken to develop power, just as the engineer in charge of an engine supplies the wood, and coal, and water, or the traveler provides food for his horse; to enable each to perform a certain amount of work requiring force. It is just as true that the power represented in the passage of albumen, fibrin, starch, and fat, in returning to carbonic acid, ammonia, water, etc., is the power or force known as life, as that the passage backward of woody fibre beneath the boiler to carbonic acid and water, *in its passage*, develops that power called heat and produces the force through which the engine moves. Further: as the woody fibre, by an alteration of surrounding circumstances, can recoil without the production of that force by which the engine moves, as in the boiling of a kettle, so a seed can recoil without producing the phenomena of life. The organization of the cell is imperfect; the conditions surrounding are altered; and the changes resulting end in putrefaction instead of life. The same principle applies to the universe in all its parts; action and reaction are opposite. Electricity is a force evolved in connection with the oxydisation of a metal, and every particle develops a certain amount of the force. This force passed through water will decompose it into its elements, and every

equivalent of zinc oxidized causes the deoxidization of an equivalent of water.

Hence, to make these principles practical, the property of irritation in a cell is its susceptibilty to recoil; its tendency to pass backward by destructive change. This is the first source of movement; the source of the motion of a cell; and nutrition, secretion, etc., etc., are the reciprocal processes to this. With this understanding of the term "irritation," he was willing to consider, with Virchow, that irritation may be considered as the starting point of inflammation.

Dr. WATTERS then passed on to the consideration of the subject in a practical point of view. What he had said heretofore might have seemed to some members entirely visionary, and they might be disposed to inquire "*cui bono.*"

Looking at man, not *merely* as an individual, isolated from the surroundings of nature, but considering him as an integer of the universe; as while standing on the earth, with tendrils extending to the sun and most distant star; and as an integer in which the universe is reflected, while composed of myriads of cells, each living for and by itself, yet for and by the whole—so is the relation of man to the universe. And considering man, not as a certain dead form, but as a form with a process, *life*, and this in *consequence* of harmony with the universe; and that this relation of nature to man, representing life, is through *destructive change*; that the food which we take in twenty-four hours is the representative of life for that time; and just as coal and water are capable of carrying an engine only so far, so precisely, with no exception, man is capable of performing so much action only in proportion as he takes in so much material in the form of food.

Now, we come to the consideration of disease. The first question to be determined is, what is health? As man is an integer of the universe, in consequence of which he exists, so each cell that is a portion of his body lives and has phenomena only in and through the whole organism.

Health, then, is this: When each individual cell of which the body is composed, living in and by itself, lives for the unity, man;

when every cell in the body performs its functions for itself and also for the man; when the liver, kidneys, glands of the stomach, etc., all perform their functions for themselves, and contribute to the unity,—then we have perfect health. Disease occurs, when from any cause, no matter what, a cell or any number of cells, in performing action for themselves, act not for the whole. The unity of the man is destroyed in and through the disharmony of the cells. When this diremption, this change from unity to diversity exists; that is, when an irritant acts on a part so as to produce greater destruction in the part than the nutrition can compensate,—then we have the phenomena of inflammation, which is then local when confined to that one part. When the whole system is affected we have the phenomena of fever, which is nothing more or less than a reaction of the system against this local disharmony.

Thus we have arrived, having gone far beyond irritability, at what irritability is, and at what irritation is; and thus irritation is nothing more or less than the susceptibility of the body to recoil. Having brought the subject to this point, he desired (for some members do not think this discussion is either useful or practical) to make one remark. Any one who is satisfied with these vague terms,—dormant vitality, irritability, etc., so far as he was concerned, might remain so.

Dr. HAMMER inquired, what is susceptibility?

Dr. WATTERS said: Organic matter, albumen, fibrin, casein, etc., placed under the conditions of life, pass into humbler and humbler forms until they return to the form of inorganic matter. The conditions of life, acting on organic matter, resolve it back to simple compounds. The susceptibility to stimulation, therefore, is the tendency of a living man, composed of these organic materials, to pass back to inorganic matter.

Dr. LINTON begged leave to inquire, whether a seed capable of germination is living or dead?

Dr. WATTERS said, If I am compelled to answer, in one word, I will say that it is *not* alive.

Dr. LINTON. Would it be any more dead if cooked?

Dr. WATTERS said: Let us illustrate this point. Let us go down to the Iron Mountain: we find there the materials for the formation of a myriad of steam engines, so far as the iron is concerned. But they are not steam engines. Make some of this iron into a steam engine, perfect in all its parts; then wood and water being placed in it, do not simply produce steam, but motion,—modes of action determined by the form of the machine. Then you have the phenomena. But take that machine and boil it, or let it rust and decay, and do you have a steam engine? No. He might make a little steam engine, and a little railroad, all perfectly formed, and have the wood and water and everything complete, except the combustion; he might keep it for a thousand years, and it would not move; but when oxygen and heat are added, then motion would occur in consequence of the recoiling. So, if an organism is life, then the seed is alive; but he held that life is a *process*. Life is not a form or state, but that process which exists when a cell under certain conditions, acting for and by itself, acts for and by the whole. It is an *act*. An act is involved in life and in disease. A ball might be shot into the heart of a man; he is diseased while he lives, but not after he is dead. Disease involves life. He was not a Neuro-pathologist, nor a Humero-pathologist, nor a Cellular-pathologist, but regarded disease as an involvement of all, just as a process, a unity of process, is life.

Disease occurs, as he had said before, when from any cause whatever, the harmony of the conditions of life is lost. Concerning Dr. Linton's question, Dr. Linton, if he understood him, defines disease "a change of organization." He would like to know how there could be an altered organization without already an altered function; or an altered function without already an altered organization. A cell being altered in disease, the actions have already changed; therefore, there can be no altered function without altered organization, and no altered organization without altered function. For illustration, suppose two children on a see-saw; the heavier one goes down, the other goes up. One goes up because the other goes down, but he does not go up *before*

the other goes down. When two opposites are necessary, we can not say of either that it is first or last.

All irritants, stumulants, and excitants are, without exception, agents, causing conditions which tend to produce greater destruction than natural. Whenever circumstances tend to produce greater change than the blood can meet and repair, then there is inflammation. When irritation occurs, blood flows, swelling, redness, increase of temperature, and pain, ensue in the part. Let us think for one moment. The blood is the common pabulum of the body. One portion is extremely vascular, another not so much so, and others not at all, according to their activity. And here we agree with Virchow, that inflammation is not due to the blood, or the nervous system, but to the recoiling of the part itself, and that the very act that calls for the blood, supplies it. In this consists the "*vis medicatrix naturæ*," as shall be seen. When an irritation is produced within certain limits, it merely produces active congestion; as in the uterus when pregnant, or in the over-used arm of the blacksmith. Within these limits it is consistent with health, but when it passes this limit it passes from excitement into inflammation. This state of things is brought about by two classes of conditions. The first class contains all those conditions which produce greater destructive change than natural,—greater destruction than nutrition can meet. The second class contains all those causes which operate in the blood, impoverishing it so that the proper nutrition can not meet the ordinary demand. To both classes belong an infinite variety of particulars, gradually shading from one class into the other.

Now then, understanding inflammation in this light, not as a thing or state, but as a process, we come to the subject most important to practical physicians,—the method of cure. In this he hoped to be clear. Let us look at nature: we have seen man, composed of many parts, as an integer in the universe. When disharmony, loss of relation between the local and organic unit, occurs, what is the process of nature by which it is repaired and restored? We have found that inflammation consists in a disequilibrium, an excess of destruction over the capacity of nutrition.

We have found this brought about by two conditions; a greater destructive change on the one hand, and an impoverishment of the blood on the other. What, then, is the rationale of scientific treatment? This. There are two indications growing out of the elements of this disease: First, to use remedies which diminish the destructive change in the part. These are of indefinite characters, and can not now be enumerated. The second indication is, to improve the nutritive capacity of the blood to meet the increased demand. And frequently the determining of these two indications is most difficult to the practitioner (as he thus steers between Scylla and Charybdis), and it is not a question to be lightly passed over.

Destructive change being the logical antecedent, it is in itself the source of vital power. You can not have life without Necrobiosis, the destructive force itself being life. The destructive change increases vital force, and instead of being entirely destructive is also recuperative. The reacting back tends to produce *life;* the vital force is in destruction. We can not, therefore, use remedies of the first class without endangering the second indication; we can not, therefore, bleed, use calomel, etc., blindly in inflammation, but only when the condition of the system is such that when using them to fulfill the first indication the powers of the system are able to bear them. It is possible that under certain circumstances this heroic treatment is good, and cures. When a part is cut with a knife, air is admitted, which is a condition of life and also of destruction. This you will admit. What are the indications? To draw the parts together, exclude the air, and keep the part cool, so that the destruction can not be much increased. You wish to place the part under natural conditions as far as possible, and so prevent inflammation. On the other hand, the inflammation itself, if excessive, causes a reactive change in the whole system, represented in the increased plasticity of the blood, which is itself the process for the cure of the inflammation. The fibrine of the blood, which is the index to its plasticity, is increased in inflammation. So far from calomel being indicated, in that it diminishes fibrine, just so far as it does

so it is contra-indicated. When an internal organ is inflamed, as the brain, heart, or lungs, the practitioners who see that inflammation is a process, not a state, use a blister; they say as a counter-irritant. But we wish to know *how* it acts. It is a counter-irritant because it acts vicariously upon the inflamed organ, increasing the plasticity of the blood, and the inflammation can only be cured by increasing this plasticity. Some time ago a case was related to the Society by Dr. Hodgen, in which the carotid artery was cut, and the patient died from softening of the brain. Dr. Hammer contended that it could not be softening, since this is only the consequence of inflammation. So it is: there is the life-death in all inflammation; but that condition may be brought about also by cutting off the nutrition. If you tie an artery you may have softening just as well as if the part, having its normal supply of blood, underwent changes too rapidly to be repaired by that amount of blood. So tubercle, which is a degeneration, is not always the result of inflammation. Hence, one predisposed to tubercular disease is poorly nourished. We recommend the patient to take exercise in the open air. The patient, already poorly nourished, is placed in conditions where still more nourishment is required, and he improves because the increased destruction is the increase of vital power. We give cod-liver oil and whisky, not to make fat, but to recoil into water and carbonic acid, and in this recoiling to generate vital force. It is customary to send these patients to a warm climate. This is absurd and entirely contra-indicated; for in southern climates less oxygen is introduced into the system than in colder climates. The object of treatment is to produce the greatest amount of vital action with the least expenditure of tissue. So far as these ideas are concerned, they indicate whisky, cod-liver oil, etc., and placing the patient in conditions of more recoiling in a cold climate. There is no necessity of producing depression by the sedative effect of cold on the system; but the patient must wear a large amount of thick, warm clothing, and at the same time breathe the cold, fresh air.

So the nervous system can not be absolutely left out of those conditions which are connected with inflammation. As electricity, in the experiment of Claude Bernard, applied to the cut sympathetic nerve caused the return of the excited part to its normal condition, so a blister, which occasions an exudation on the surface of the body, may, through the nervous connection, tend to the same result. Thus a blister is indicated, not only because it increases the plastic character of the blood, but also that it may thus act beneficially through the nervous system.

Dr. LINTON said, that the hour being late he would not trespass long on the patience of the Society. After listening to Dr. Watters for an hour on the previous evening, and for two hours this evening, he had came to the conclusion that there was some truth in what he had said, though it was difficult to grasp. The views of both Dr. Hammer and Dr. Watters may be true, as there seems to be not much contradiction between them. They seem to view the same subject from different sides. He differed, to some extent, from both. He did not pretend to understand entirely Dr. Hammer's position, as he had not heard his remarks, and only judged from the report of his speech read at the previous meeting. If he understood Dr. H., he had said that the originality of Virchow's theory consisted in this: the passivity of the blood vessels in inflammation; he maintaining that they are perfectly passive in this process. He is perfectly right in this, for we can not conceive of such a thing as active dilatation. If he understood Dr. H., he said that the doctrine was original with Virchow. He maintained that this was original with neither Bernard or Virchow, but was the doctrine which he had advocated and taught for twenty years, and thought that it was expressed in his work on Pathology. It is also enunciated in Billings' Principles of Medicine, published twenty-five years ago. The experiments of Cl. Bernard prove the same thing, and Virchow has adopted it. There is no doubt that the vessels are as passive in active congestion as in passive. There is another point brought forward by Virchow as original,—that there is no such thing as a development of cells in exuded plasma, and that the manner in

which cells are formed is from other cells. All the members are familiar with these two doctrines; one, that cells may be generated in a hyaline exudation destitute of form. Virchow contradicts this, and maintains that cells are never formed except from previous cells. That is original, it is new,—but probably not true. He had no objection to their being generated in this way; but the question is, *are* they thus generated? It is a simple question of fact; has any one observed it? If so, it is a fact; it is no question of theory. Certain authors,—Bernard, Hilbert, Mandel, Pelleter, and others, state they have observed cells originating in hyaline plasma containing no molecules. It has been observed by one author that cells are generated in serum taken from a blistered surface and placed in a phial. If this is true, the development of cells from exudations is established. If the other doctrine is true, it does not conflict with this. It may also be true. If men of truth and science make both these statements, we must believe them, and it does no good to say that cells are always induced one way or the other. These are the two points which he wished to consider; the passive dilatation of the blood vessels, and cell generation.

Dr. LINTON proceeded to make other comments on previous remarks on this subject, which will be contained in the next report of the Transactions of the Society.

ANNUAL MEETING OF THE AMERICAN MEDICAL ASSOCIATION.

This body met May 1st, at 11 o'clock, in the main hall of the Concordia Opera House, on Eutaw street, near German street, Baltimore, there being about two hundred and fifty delegates present. The President, Dr. D. H. Storer, of Boston, took the Chair. After being called to order, a very impressive prayer was offered by Rev. Dr. Spees.

The Chair stated that the first business under the rules was the reception of communications from the Committee of Arrangements. Dr. C. C. Cox, of that Committee, in an eloquent address, gave a warm welcome to the

Association, and hoped that when the short stay of the members is ended, they will have cause to retain kindly remembrances of the Monumental City. He expressed his regret that so few delegates from the South were present, and hoped now that peace has come they will again return and aid the Association with their learning and experience in the great work the profession has before it. He paid a high compliment to the surgeons on both sides during the late war, and referred in pathetic terms to the many learned men who have been taken away by death since the Association met in Baltimore eighteen years ago He closed by again warmly welcoming the visiting brethren.

The hour of meeting during the session was fixed at 9 o'clock, A. M.

Under a suspension of the rules, Dr. C. C. Cox presented several papers in reference to the case of Dr. Montrose A. Pallen, of St. Louis, whose name was last year erased from the list of delegates, on charges preferred against him, showing clearly the innocence of Dr. P., and moved that they be referred to the Committee on Medical Ethics.

Dr. Toner, of Washington, moved, as an amendment, that the Association take up the subject without any reference to the Committee.

Dr. Ordway, of Boston, advocated the amendment, and stated that as the action of the previous year had been done without reference to a committee, he thought that the reparation should be as decided and prompt in the present instance. Dr. O. further remarked that as one of the protestants to the action of the Association last year, it was eminently proper that this apology should be made by a direct vote.

Dr. Davis, of Illinois, supported the original motion, on the ground that, as the action of the Association in this matter was wide spread upon the records, it was only just to Dr. Pallen that his vindication of the charges should be as full and thorough on the journal as possible. By a mere motion to rescind this would not be done, and through a committee a full report and thorough exoneration would be made public.

Dr. Cox stated that he had an interview with Dr. Pallen on Monday, and that Dr. P. preferred that the matter should be referred.

Dr. Owens, of Maryland, asked for information with regard to the case, whereupon the Chair stated, that at the session held last year in Boston, charges had been preferred against Dr. Pallen, then a member of the Association, of disloyal conduct and practices, and a resolution had been there passed to erase his name from the list of members, and the question now was to restore Dr. Pallen to his membership, the statements against him having been proved to be unfounded.

The main question having been called, the papers were referred to Drs. Hooker, Brinsmade, and Davis, as a Committee on Medical Ethics.

A list of names of delegates, from the Committee on Credentials, was read by the Secretary.

Under the rule for the reception of members by invitation, James H. Rives, of Fairmount, West Virginia, was received.

Dr. Thomas E. Bond moved that the committee to whom was referred the case of Dr. Pallen, be instructed to report the expression of their profound regret at the Association being hurried into its unjust action to Dr. Pallen, and that they hope that Dr. Pallen will accept this acknowledgment as an expression of a frank apology for the great wrong done him; which, together with an amendment, was laid on the table.

Dr. Storer, President of the Association, read his annual address, which was heard attentively, and on motion was referred to the Committee on Publication. A unanimous vote of thanks was tendered to Dr. Storer for his excellent address.

The Committee on Ethics, to whom was referred the case of Dr. Pallen, announced their readiness to report, which was read, and speeches made thereon by Drs. Tyler, Bond, Ordway, and Owens, the last named moving that the report be recommitted, with instructions to incorporate Dr. Bond's resolution, which had been previously tabled.

Dr. Hooker, from the Committee, then presented the following amended report, with the preamble and resolution, which were unanimously adopted:

"The Committee to whom were referred the papers in relation to the expulsion of Dr. Montrose A. Pallen, at the meeting of the Association in Boston, respectfully report that they have examined the documents and evidence referred to them, embracing papers indorsed by Lieut. Gen. U. S. Grant, the Vice-Consul of the United States at Montreal, and many citizens of Missouri, and are fully satisfied that the statements on which his expulsion was based were entirely unfounded, and therefore, regretting the injustice done both to Dr. Pallen and the Association, we recommend the following resolution:

Resolved, That the preamble and resolution adopted by the Association at their annual meeting in Boston, June 1865, expelling Dr. Pallen, be hereby rescinded, and that Dr. Montrose A. Pallen be restored to his previous membership in the Association."

Dr. Ordway sustained the same, and moved that a committee of three be appointed to meet Dr. Pallen and inform him of the unanimous action of the convention.

Dr. Owens moved as an amendment that Dr. Cox be Chairman, which was accepted by Dr. Ordway.

The Chair then appointed Drs. Cox, Ordway, and Sayre.

The committee soon after returned with Dr. Pallen, who was conducted to the platform amidst applause, and presented to the Association as perfectly exonerated from all charges. Dr. Pallen, in reply, stated that he was deeply gratified at the action of the Association; that its action in regard to him had almost overwhelmed him, but that, whilst buffeting with the waves of infamy and disgrace, he was comforted by the bright light of conscientious performance of duty. The man who would have dared to reproach him, with even a hint of poisoning the Croton reservoir, would have been spurned with contemptuous indignation. On the battle-fields of

the war he had been stayed by no danger in the performance of his professional duty, and whether the wounded man was clad in gray or in blue, he had ministered to him, regardless of the time or place. He returned his thanks again to the Association, and also to Dr. Cox for his manly conduct, and closed with the expression of his gratitude for the warmth of his reception.

The call of special committees and volunteer papers on professional subjects for the purpose of being referred to appropriate sections was then made, lasting some time.

Dr. Jewell announced the presence of Dr. Marsden, from Canada, and moved that he be received as a permanent member, and be invited to take a seat on the platform, which was agreed to.

On motion, Dr. E. Brown-Séquard, of Boston, was invited to address the Association to-day, at 11 A. M., on the treatment of nervous disorders.

After some business announcements, the Association adjourned until 9 A. M., the various sections meeting during the afternoon to prepare business.

In the evening there was a promenade concert and entertainment to the visiting delegations, at Concordia Hall, given under the auspices of the Committee of Arrangements, which was, notwithstanding the rain, fully attended, many ladies being present. The next evening private soirees were to be given the members of the Association by Dr. C. C. Cox, No. 23 McCulloh street, Dr. T. S. Bond, 82 Read street, and Surgeon Joseph Simpson, U. S. A., No. 40 Calhoun street. The entertainment tendered by the Mayor and City Council will follow.

SECOND DAY, May 2. — The Association was called to order by the President, Dr. D. H. Storer, at 9 A. M.

The Committee on Epidemics, Meteorology, &c., having been called upon, Dr. Davis stated that Dr. Hamill, of Ind., had presented a report, which he had taken to the Section on Epidemics, &c.

Dr. Cox made an additional report from the Committee of Arrangements on Railroads, that invitations had been received from Drs. Smith and Donelson, for the members to visit their houses that evening. He also recommended the following gentlemen as members by invitation: Drs. John A. Reed, W. Whitridge, L. M. Eastman, of Baltimore; Peter Parker, of China. They were elected.

On motion of Dr. Davis, the order of business was suspended.

The report of the Committee on Publication was read and accepted.

On motion of Dr. Sayre, of New York, the Publication Committee were authorized to enforce strictly the rules in regard to proofs, etc.

The Treasurer then read his report, which was referred to the Committee on Publication.

On motion, the order of business was resumed.

On motion of Dr. Davis, a recess of fifteen minutes was taken by the Association, to allow of the appointment of members of the Nominating Committee.

The Nominating Committee.—On the resumption of business, the following members of that Committee were announced: J. C. Weston, Maine; J. C. Eastman, N. H.; Wm. McCollom, Vt.; J. R. Bronson, Mass; D. King, R. I.; W. Woodruff, Conn.; J. C. Hutchison, N. Y.; W. Pierson, Jr., N. J.; H. F. Askew, Del.; John L. Atlee, Pa.; J. J. Cockrill, Md.; M. A. Pallen, Mo.; N. S. Davis, Ill.; W. Lockhart, Ind.; J. M. Witherwax, Iowa; N. R. Bozeman, Ala; C. M. Stockwell, Mich.; H. Van Dusen, Wis.; T. A. Atchison, Tenn.; G. Fries, Ohio; G. Tyler, D. C.; W. M. Charters, Geo.; Josiah Simpson, U. S. A.; Ninian Pinkney, U. S. N.; Greensville Dowell, Texas.

Dr. W. Hooker offered the following resolution, which was unanimously adopted:

"*Resolved*, That no report or other paper shall be presented to this Association unless it is so prepared that it can be put at once into the hands of the Secretary, to be transmitted to the Committee on Publication."

Dr. Wistar, of Pa., offered the following, which was adopted:

"*Resolved*, That Drs. Grafton Tyler, W. P. Johnson, and James M. Toner, of D. C., be a committee to procure a room in the Smithsonian Institution for the preservation of the Archives of the Association."

The Committee on Medical Education not having prepared a report, Dr. J. F. Hibberd offered instead thereof the following preamble and resolution, and moved that it be adopted as the sentiment of the Association:

"WHEREAS, Two-thirds of the medical colleges of the States of Ohio, Michigan, Illinois, Iowa, Missouri, Kentucky, and Tennessee, by delegates in convention assembled in Cincinnati, on the 24th of April ult., did, by resolution unanimously adopted, declare their willingness to make their annual college sessions to continue for six months, and to establish a uniform rate of fees, if the other principal colleges of the country will coöperate; now, therefore,

"*Resolved*, That the American Medical Association hereby expresses its warmest approbation of the action of the above recited colleges, and expresses the hope that every medical college in the Union will concur in the proposition thus made."

On motion of Dr. Taylor, of Iowa, its consideration was postponed till 11 A. M., on Thursday, to be acted upon in Committee of the Whole.

Dr. C. A. Lee, of New York, commenced reading his report upon Medical Literature. He divided up his subject as follows: I. Periodical Medical Press. II. Medical Literature of the War. III. Literature of the Sanitary Commission, and of Sanitary Sciences. IV. State and County Society Transactions. V. Literature of Special Subjects and of Specialties. VI. Literature of Pharmacy and Materia Medica. VII. Of Vital Statistics. VIII. Of Life Assurances. IX. And of Introductory Lectures.

He was interrupted at 11 for the regular order of business, which was the lecture of Dr. Brown-Séquard, on the Treatment of Functional and Organic Diseases of the Nerves.

On motion of Dr. Raphael, of N. Y., the thanks of the Association were tendered to Dr. Brown-Séquard for his interesting, able, and eminently practical lecture, and he was requested to furnish an abstract for publication.

Dr. C. A. Lee then resumed the reading of his report.

After this had continued for some time, on motion of Dr. Toner, the further reading was discontinued, and the paper referred to the Committee on Publication.

Dr. Samuel D. Gross, Chairman of the Committee on Medical Education, reported that he had not prepared a report, and asked that the Committee be discharged, which was granted.

Report of Prize Committee.—Dr. E. Eliot, Secretary of the Committee on Prize Essays, read the report of that committee.

On breaking the seals, Dr. W. F. Thoms, of New York city, was ascertained to be the author of the "Essay on Health in Cities," etc., and was entitled to the first prize, and Dr. S. R. Percy, of N. Y., on "Digitaline," etc., to the second.

On motion, the paper on Angular Curvature of the Spine, was referred to the Section on Surgery.

The report of the Committee on Medical Ethics having been offered, it was made the special business for 9.30 on Thursday.

Dr. Marsden, of Canada, having been announced as desirous of making some remarks on Cholera, on motion, it was agreed that he should follow immediately after the report on Medical Ethics.

Dr. Cohen offered a paper on Paralysis of the Vocal Chords and Aphonia, etc. Referred to the Section on Surgery.

Dr. H. R. Storer offered a paper on the "Clamp Shield," an instrument designed to lessen the dangers of extirpation of the uterus by abdominal section.

Dr. Bozeman, of Ala., was introduced to the Association, and on motion of Dr. Holton, he was made the member of the Committee on Nominations for Alabama.

Dr. Askew offered the following resolution on the death of Dr. Couper, which was unanimously adopted:

"WHEREAS, We have heard with profound regret of the death of our deservedly esteemed friend and associate, James Couper, M. D., of Delaware, late Vice-President, and one of the founders of the American Medical Association; and whereas, we desire to express our high appreciation of his worth as a man, and valuable and untiring energy in the cause of medical science; mild, modest, and unassuming, of devoted piety, he was firm, constant, and reliable; a strict adherent to the ethics of the profession, he occupied a front rank, and died beloved, respected, and lamented by all who knew him.

"*Resolved*, That in the death of Dr. James Couper we have lost a friend and brother, and that we sincerely and deeply condole with his sorrow-stricken widow and family, and that the Secretary be authorized to forward a certified copy of these resolutions to his family."

Dr. Toner, of D. C., offered the following resolution, which was adopted:

"*Resolved*, That instead of yearly reprinting the list of members of the American Medical Association with the Transactions of the same, the Secretary be instructed to prepare and have printed in pamphlet form, a triennial alphabetical catalogue, containing the Constitution of the Association, and a list of members, with their full names, designating their residences, the year of their admission, arrearage of yearly dues, the offices they may have held in this body, and in case of death or resignation, the year, and distribute the same among the contributing members."

On motion, the resolution was referred to the Committee on Publication.

Dr. J. C. Hughes, of Iowa, offered a paper on Lithotomy, which was referred to the Section on Surgery.

Dr. Taylor, of Iowa, introduced a resolution for the appointment by the President of the Association of a member from each State, to memorialize Congress for an appropriation to publish the reports and documents of the Surgeon-General of the United States.

Dr. Pallen recommended that the reports and documents of the like character connected with the rebel army be also referred to the same committee for access to the same. Dr. Pallen, after some discussion, withdrew his amendment.

The original motion was carried.

It was then moved that the President announce said committee on Thursday morning.

The meeting then adjourned.

THIRD DAY, May 3d.—The Association was called to order at 9 A. M., by the President, after which the announcement of the members of the Committee to memorialize Congress on the publication of the surgical history of the war was made.

Dr. C. C. Cox, of the Committee on Necrology, reported progress, and on motion of Dr. Hibbard, permission was given the reporter to send the report, when ready, to the Committee on Publication.

The Death of Professor Joseph M. Smith, of New York.—Dr. Alfred C. Post offered the following, which was unanimously adopted:

"*Resolved*, That the Association has heard with sincere regret of the death of its late distinguished member, Joseph M. Smith, M. D., of New York:

"*Resolved*, That we cherish his memory as that of a learned and skillful cultivator of medical science; an able and successful teacher and writer; an upright and honorable man; and a patriotic and public-spirited citizen.

"*Resolved*, That the Secretary communicate to the family of the deceased an expression of our sympathy with them in their bereavement."

Dr. C. A. Lee rose to speak to these resolutions, which he did with much feeling. He hardly thought that it was necessary to say anything in regard to the life or character of such an excellent and well-beloved man, but as

he had been intimately acquainted with him for over thirty years, he did not think it out of place for him to say a few words. After referring in an appropriate manner to his acquaintance with the deceased, he remarked "that a more pure, upright, and conscientious man I never knew, particularly with reference to his intercourse with medical men. When I think of the great loss we have sustained in him, I am at a loss to express myself."

Dr. J. S. King, of Natchez, Miss., forwarded a communication to the Association, stating that he was engaged in the compilation of the mortuary and similar statistics of the principal cities and towns of the country, and requesting that physicians would transmit to him such information upon those subjects as they could gather in their respective localities.

The Secretary read a communication from the Dubuque (Iowa) Medical Society, requesting the erasure of the name of Dr. Asa Horr.

On motion of Dr. Jewell, the request was granted.

Dr. Mayburry, on behalf of the Committee on Publication, to whom Dr. Toner's resolutions were referred, reported the following as a substitute, which, on motion, was adopted:

"*Resolved*, That instead of yearly reprinting the list of the members of the American Medical Association, the Committee on Publication be instructed to prepare and print with the Transactions, an alphabetical catalogue triennially, containing a complete list of the permanent members, with their names in full, designating their residences, the year of their admission, the offices they may have held in the Association, and in case of death or resignation, the date thereof."

Dr. Mayburry also presented the following, which, on motion, was referred to the Committee on Ethics:

"WHEREAS, Medical organizations, such as National, State, and County Societies, are believed to be absolutely necessary to preserve the honor of the medical profession, and to keep alive social and fraternal feelings among the members thereof, as well as an important means of promoting medical knowledge and elevating the character of the profession; therefore,

"*Resolved*, That it is with sincere regret that we, the members of the Montgomery County Medical Society of Pennsylvania, learn that some honorable members of the faculties of our medical colleges in Philadelphia, and elsewhere, have kept aloof from the county societies on which rest both State and National organizations, thus ranging themselves on the side of those whose unprofessional conduct, or low standard of medical attainment, or disregard of medical etiquette, prohibits them from membership in those societies.

"*Resolved*, That as graduates of the University of Pennsylvania, Jefferson Medical College, and Pennsylvania Medical College, we have a high regard for the teachers of those institutions, and feel that they owe it to the profession, and to our Alma Maters, to give their hearty support to medical organizations in general, and especially to the County and State Medical Societies.

"*Resolved*, That although colleges are entitled to representation in the American Medical Association by one or more of their professors, we are decidedly opposed to any college, or any other medical organization, being represented by a professor who is not a member of a county society.

"*Resolved*, That the Corresponding Secretary of this Society be instructed to report these proceedings to the Philadelphia County Medical Society, and that our delegate be charged to lay them before the American Medical Association at the coming meeting, to be held in Baltimore, on the first day of May next, as well as before the Medical Society of the State of Pennsylvania, at its next meeting, to be held at Kingston, Luzerne county, on the thirteenth day of June ensuing.

E. SMYSER, *Cor. Secretary.*

W. P. ROBINSON, *Pres't Montgomery Co. (Pa.) Med. Society.*

The Report of the Committee on Ethics.—Specialties in Medicine.—Dr. Worthington Hooker offered the majority report, and in the main took the ground adverse to exclusive specialties. He divided up the subject into exclusive and partial specialties. In reference to exclusive specialism, he maintained that local affections were apt to be unduly estimated, to the exclusion, perhaps, of other parts of the system that were of more importance in the production of a particular disease; that the diseases cured by a specialty are magnified in their importance; that specialists too frequently undervalue the treatment of diseases by the general practitioner; that there is a temptation to employ undue measures to obtain notoriety; and that he is further tempted to charge unduly large fees. The field of medical practice was so large that the profession was always willing to seek advice from those who had devoted attention to particular subjects; but this should not encourage exclusive specialism. The specialty should be a natural outgrowth from the general practice, and should never be separated from it. If this were so, a full, frank, and free intercourse would be had between the specialists and general practitioners. The means availed of by the specialists to bring this fact before the public should be ordinary, and not extraordinary. There should be neither advertisements nor puffs in the newspapers. The professor in a school has been chosen for it by those who are competent to discuss his merits for that position; if he were by himself to place before the public the fact that he is specially skilled in the branch taught by him, he would come under this censure.

The report was well drawn up, and claimed the undivided attention of the members.

Dr. Kennedy, of New York, followed with a minority report, stating that he would read it in the absence of the writer. The writer believes that the whole tendency in every department of science is towards specialties. Science has been advanced during the last century by this course. Recently this tendency has shown itself in the persons of certain practitioners who resign all general practice, and confine themselves to the specific department they have chosen. No Association can object to the advertisement in such cases, unless it is of a mountebank character. The report was signed by H. I. Bowditch.

The subject was then discussed by Drs. H. R. Storer, of Boston; Worthington Hooker, of New Haven, and others; but the hour of 11 having arrived, Dr. W. Marsden, of Quebec, was introduced, and proceeded to address the Convention on the subject of cholera connected with quarantine.

THIRD DAY, May 8d (*Concluded*).—*Cholera and Quarantine.*—Dr. Marsden, of Quebec, according to previous appointment, made some remarks upon cholera. He commenced by stating his belief in the communicability of cholera, and the efficiency of a rigid quarantine. He had witnessed the first case that had occurred on the American continent, and since that time had given much attention to the study of the disease. He was now convinced that every case of cholera could be traced to infection, and that the proper soil for the propagation of the disease was found in filth, and the neglect of the ordinary sanitary precautions. He believed that all clothing from patients suffering from the disease should be destroyed, and thus be prevented from spreading the disease. He believed that isolation would prevent the appearance of the disease in any community, and related an instance in point which had made such a strong impression upon him that he was caused to think first of his plan of quarantine. It seems that a schoolmistress, in a locality where cholera threatened to make its appearance, consulted the doctor on the best course to pursue. He advised her, as soon as the disease should appear, to isolate the school from the rest of the town, by closing her gates and doors. This was done, and not a single case of cholera occurred within the walls. Dr. Marsden next gave the members a detailed account of his system of quarantine.

"1. The cholera quarantine station shall be divided into three separate and distinct sections or departments.

"2. Each of these three sections or departments shall be isolated and separated from the others by a *cordon*, a portion of neutral ground of not less than one hundred feet wide.

"*a.* One of these sections or departments shall be appropriated to the use of the sick, and shall be the hospital department.

"*b.* The next, or central section or department, shall be devoted to the use of passengers not having had cholera, but from infected vessels.

"*c.* And the third or healthy section or department shall be appropriated to the use of the healthy, who have been removed from the central department, after having performed quarantine there.

"*A.* In the first section or department there shall be three separate and distinct hospitals, besides a convalescent shed or hospital.

"*a.* The one for confirmed cases of cholera, to be called the *cholera hospital.*

"*b.* Another for cases of choleraic diarrhœa, or other premonitory symptoms of cholera, to be called the *hospital for cholerine.*

"*c.* The third for all other diseases not cholera or cholerine, but coming from on board infected vessels, or vessels having had cases of cholera on board, to be called the *general hospital.*

"*B.* The next, or central section or department, shall be the primary quarantine department, and shall be appropriated to all persons who are not sick, but come from vessels having had cholera on board, and wherein every case on landing shall undergo inspection, washing, cleansing, and purifying, both of persons and personal effects. There a quarantine of four days shall be performed, at the end of which period of time all such persons as continue in sound health shall be removed to the final quarantine department; and any that may fall sick or be threatened with sickness during the four days of probation, shall, as soon as detected, be removed to the proper hospital, in the hospital department. There also the healthy inmates shall be removed daily to a new locality, thus occupying four different habitations during their sojourn.

"*C.* The third, or healthy department, shall be the final department, and shall be for all cases coming from the primary quarantine department, after having been cleansed, washed and disinfected, and after having undergone the four days' quarantine; and here a further quarantine of six days shall be performed (excepting cases coming from the convalescent hospital or shed, hereinafter provided for); making in all ten days of quarantine, when all persons continuing healthy shall be discharged from quarantine, and be removed from the station. If any premonitory symptoms or other cases of sickness occur in this department during the six days of quarantine, they shall, as soon as discovered, be removed to the proper hospital, in the hospital department.

"No communication will take place with the hospital department, except through the central or primary quarantine department, for which purpose a passage, unfrequented by the persons undergoing quarantine, shall be set apart and reserved."

Dr. Lee moved the thanks of the Association to Dr. Marsden for his interesting and practical address, and the request of the body that he furnish it with a digest of his communication.

Dr. Bond amended, that those papers accompanying the lecture be commended to the city authorities, and the authorities having such matters in charge throughout the country, for their action.

Dr. Jewell thought the matter should be further investigated, and moved its reference to the Section on Hygiene, to meet that afternoon.

The special business of the day was suspended to allow the Committee on Nominations to report.

The Officers for 1866-7.—*President.*—H. F. Askew, Delaware.

Vice-Presidents.—W. K. Bowling, Tennessee; J. C. Hughes, Iowa; H. I. Bowditch, Massachusetts; Thos. C. Brinsmade, New York.

Permanent Secretary.—Wm. B. Atkinson, Pennsylvania.

Treasurer.—Caspar Wistar, Pennsylvania.

Assistant Secretary—W. W. Dawson, Cincinnati.

Committee of Arrangements.—Drs. John A. Murphy, James Graham, R. R. McIlvaine, J. P. Walker, ——— Unsicker, William T. Brown, William B. Done, Cincinnati.

Committee on Medical Education.—Drs. S. D. Gross, D. F. Condie, John Bell, H. J. Bigelow, Charles A. Pope.

Committee on Prize Essays.—Drs. Francis Donelson, Maryland; Josiah Simpson, U. S. A.; C. C. Cox, Edw. Warren, W. C. Van Bibber.

Committee on Publication.—Continued.

Committee on Medical Literature.—Drs. A. C. Post, James Anderson, H. D. Noyes, T. G. Thomas, Stephen Smith,—all of New York.

Committee on American Medical Necrology.—Continued, with the additions: Dr. Wood, Delaware, substituted for Dr. Couper; John L. Callender, in place of Dr. Bowling, Tenn.; John Blaine, in place of Wm. Pearson, N. J. The following were added: Drs. R. D. Arnold, Georgia; Lopez, Alabama; G. Dowell, Texas.

Committee on Climatology and Epidemics.—Continued, with the additions: H. Jones, in place of C. L. Allen, Vermont. The following were added to the committee: Drs. U. Harris, Georgia; G. Engelman, Missouri; R. Miller, Alabama; Fenner, Louisiana; G. Dowell, Texas.

All special committees are to be selected by the sections to which the subjects relate.

The next place of Meeting.—The place recommended for the next annual meeting of the Association, is Cincinnati, Ohio, on the first Tuesday in May.

On motion of Dr. Ordway, of Boston, the report of the committee was adopted.

On motion, the Association went into a Committee of the Whole to discuss the resolution offered by Dr. Hibberd, having reference to extending the time for the course of study in the different medical colleges.

The whole matter was earnestly discussed by Drs. D. H. Storer, Worthington Hooker, Wright, of Ohio, Davis, of Illinois, and others, and resulted in the passage of the following resolution, offered by the last gentleman:

"*Resolved*, That the Association most earnestly request the medical colleges of the country to hold a convention for thoroughly revising the whole system of medical college instruction for the purpose of establishing more uniformity of time, and a more systematic course of instruction for the whole."

The report of the Committee of the Whole was adopted, and a committee consisting of Drs. Davis, W. Hooker, S. D. Gross, M. B. Wright, and Shattuck, was appointed.

Dr. C. C. Cox read the report "On Rank in the Army," which was referred to the Committee on Publication.

Dr. Cox then offered the following, which was adopted:

"*Resolved*, That the President of this Association bring before the notice of the Military Committtees of both Houses of Congress, at as early a period as possible, the present status of medical men in the military service of the United States, and urge upon them that in the army medical bills, under

consideration of Congress, the interests of the medical profession shall be so regarded that the medical staff in the service shall, numerically considered, receive the same rank and command as officers in other staffs of the army are justly entitled to."

The committee appointed to act on the foregoing resolution were Drs. D. H. Storer, C. C. Cox, T. Antisell, W. P. Johnson, and C. L. Allen.

On motion of Dr. Cox, the following members, by invitation, were elected: W. D. Stewart, Va.; W. S. Forward, H. W. Stump, and J. L. Chaplain.

A committee was appointed on the subject of Fracture of the Spine, of which Dr. Brown-Séquard was made Chairman.

On motion, Drs. A. C. Post, T. Antisell, and J. L. Atlee were added to complete the Committee on Medical Ethics.

Specialties.—On motion, the report of the Committee on Ethics, which had been laid on the table, was called up.

On motion of Dr. Toner, the resolution attached to the minority report was omitted, and the reports were both adopted.

A motion to reconsider next prevailed, and the resolution was added to the minority report, and referred as before.

Dr. Homberger, of New York, made a request to offer a personal explanation, which, after considerable discussion, confusion, and sensational speaking, was granted.

On motion of Dr. Sayre, it was agreed to hold an adjourned meeting at 5 P. M., to discuss the subject of cholera.

A communication from Dr. McGee, "On Periosteal Flap Amputations," and one from Dr. Elsberg, New York, "On Diagnosis of Diseases of the Larynx," received, and both referred to the Section on Surgery.

The Association then adjourned until 5 P. M.

THIRD DAY.—AFTERNOON SESSION, May 4.—At 5 P. M., according to previous adjournment, the Association met, and after being called to order, resolved itself into a Committee of the Whole, choosing Dr. Davis as Chairman.

The subject for discussion as previously announced, was—

Cholera.—Dr. Sayre, of New York, opened the discussion. He considered that the disease could not reach here unless it was brought here; that it could not be generated here. It multiplies its ravages when filth and uncleanliness abound, and is generated in a sandy, level country, beneath a temperature of 128 degrees. There the decomposing animal and vegetable substances originate this peculiar poison. He believed that it accompanied the individual, and that it did not travel by atmospheric power. He thought that the Government was responsible for permitting the disease to get into the land. A rigid, proper quarantine, universally adopted by the General Government in combination with the British Provinces, would, in his opinion, prevent its admission to our continent. We had no quarantine, rightly considered. The disease in 1849 did not originate in Baxter street,

New York, but took its origin from an infected person who escaped from quarantine. The cabin passengers escape because the disease has not traveled two hundred feet nor ten feet from the steerage to the cabin. He remarked that he did not believe in mysteries, but wished to understand facts in his own way. If the valuable information that he had received from Dr. Marsden were put into practical application by the General Government, he believed that millions of money and millions of lives would be saved.

Dr. Linton protested against the doctrines advanced that morning and evening. We had medical journals through which we could discuss this subject a long time before the cholera would get here, and a long time before quarantine could prevent its getting here. "Who can believe that cholera could have been prevented from coming here in 1849? I do not believe it is any more contagious than intermittent fever. I am certain that nine-tenths of the physicians of this country are convinced of this fact. I say to the citizens of New York, Baltimore, and Canada, you may have no fears of the cholera. If it comes, it will arise in your midst. Cholera is not a disease (!!)." He did not believe that there was any truth in the doctrine of contagion. "Cholera breaks out in ships after they are six weeks at sea. I saw a case in St. Louis two months ago. Where did the Asiatics get it from?"

Dr. A. N. Bell, of Brooklyn, N. Y., thought the facts of Dr. Marsden inconsistent with the results of observation. Dr. M. had traced it first from a brig in Liverpool. He did not say that cholera existed in Liverpool at the time. Dr. B. believed cholera could be traced to various places other than Asia. "If cholera is contagious, it takes various roundabout ways of making short journeys. It took an exceedingly roundabout way to the principal cities of Europe. Of the present epidemic, it is said the Mecca pilgrims first had cholera. The evidences I have collected are against strict quarantine. The passengers of the Atlanta were detained at quarantine; no cases occurred among the well passengers after they left the ship. Of all the things likely to originate cholera, none are equal to a crowded, filthy ship. None of the passengers or things of the Atlanta were taken to Ward's Island Hospital. I would protest against the endorsement of any restrictions against persons as advised by Dr. Marsden. The detention of well persons can never protect us from any disease. Our protection is in our clean houses, for cholera often leaps over healthy residences. The action of the health officers at the New York quarantine has been fatal to well persons, and has tended to ward off investigation of the places where cholera originated."

Dr. John L. Atlee, of Pa., said that it was difficult to know the facts in large commercial cities. There are a thousand avenues to such cities as New York and Boston; but in the inland districts we are more likely to reach a better observation of facts. In 1832 I was in the midst of cholera, at Lancaster County Hospital, Pennsylvania. I believed that cholera and yellow fever were diseases independent of any idiomiasmatic conditions of

the atmosphere. In July or August, 1854, a certain peculiar condition of the atmosphere existed. The water of the Susquehanna was very low, and the water of the basin very filthy, yet there was no cholera. There were, however, some cases of bilious and intermittent fever. One day a car of emigrants came from Philadelphia to Columbia; two or three of the passengers were ill, and were put upon the platform. Four gentlemen seeing them there at the point of death, conveyed them to a shed. In the next twenty-four or forty-eight hours not one of them was living. In two or three days the cholera prevailed in Columbia. In the Lancaster County Hospital the winds were from the south. We had no cholera. A few days after the cholera broke out in Columbia, an emigrant reached there afflicted with cholerine. Shortly after, two or three cases of cholera existed. The same train conveyed the cholera to Pittsburgh. Passengers came to the vicinity of Lancaster, at a place called Paradise. Their effects were sent to Lancaster, in a high and healthy location. The relative who washed the clothes died of cholera. It is a contagious disease. Why did it not spread? Why did not small pox spread? There is an atmospheric condition favorable to the development of the disease. The result of observations in Sweden was that it had been conveyed there by the clothes of sailors. I think Dr. Marsden is right and Dr. Sayre is right, and our friends in Philadelphia must come to the same conclusion if they wish to preserve that metropolis from the ravages of the cholera."

Dr. Sayre said the quarantine law of New York, as now enforced, is a disgrace to civilization. Dr. Carnochan himself, and others, saw the cases on Ward's Island, and they all came to the conclusion that they were not cases of cholera.

Dr. A. N. Bell remarked that Dr. Geo. Ford insisted that the Ward's Island cases he treated were those of cholera.

Dr. Sayre then quoted from Dr. Ford's official statement in the annual report of the Commissioners of Emigration, in which he (Dr. F.) stated on page 52, that those "twenty-seven deaths were caused by *diarrhœa* and dysentery." This was the *official* statement of Dr. Ford.

Dr. Marsden said that cholera followed human travel. He adduced other facts to demonstrate its contagious character. It is infectious in person and personal effects. He urged the necessity of guarding against any communication between the infected and the well. Equanimity, cleanliness, and temperance were the three great adjuncts to the quarantine.

Dr. Jewell, of Pa., said: "I have been charged with disseminating cholera. I have done all I could to prevent its entrance to Philadelphia. Cleanliness and ventilation will do much to that end. We have been engaged at that during the past winter. I do not believe in quarantining healthy people. That would be disseminating the disease by giving it to the well persons on vessels where cholera existed. We had the epidemic in the summer of 1849 in Philadelphia. It began in four different portions of the city. The first case was at Richmond, the second at Eighth and Spring-Garden streets, the third in Moyamensing. These were all in the center of

the city, except at Richmond, and remote from the Delaware. The filth produced the disease in Richmond and along the Delaware. In 1832, the first case was on the Schuylkill, in a canal boat that came down from the upland country. There had been no foreign arrival in Philadelphia. It came from a poisoned atmosphere. In 1849 no flies were living. In Wheeling the birds died. The doctrine of contagion is dangerous, and will deprive the sick of assistance. Small-pox does spread, and if we had not vaccination it would spread more than it does. Contagion and infection are distinct. Contagion is the principle communicating the disease from one person to another. It is not so with cholera. There were no cases of contagion in 1832 or 1849. No vessels arrived with cholera on board. They may have arrived after the disease appeared. I am sorry the resolution was introduced. Next year we will be better prepared to test the value of Dr. Marsden's information. The poison of cholera will increase rapidly by contact with filth. It is only by purification of the city that cholera can be prevented."

Dr. Lee followed with some brief remarks sustaining the views of Dr. Marsden, and maintaining that it was contagious under certain circumstances. Certain neighborhoods of a very filthy character were not attacked until emigrants came there.

The Committee of the Whole rose, and the Association adjourned without further action.

Entertainment by the Corporate Authorities.—The corporate authorities of the city gave an entertainment to the members this evening, at which were present all the notabilities of the city, including the principal officers and members of the City Councils. The entertainment was prepared in the most generous and munificent manner, and reflected infinite credit on the donors. Between four and five hundred gentlemen were present. The supper was called at nine o'clock. A band of music, stationed in the gallery, initiated the occasion with an appropriate air, and at intervals in the course of the evening, performed all the national hymns and songs. After discussing the substantials of the bill of fare, the customary toasts were given by Dr. J. Faris Moore, toast-master, and suitably and eloquently responded to by gentlemen of the municipal government of the city, and the officers and members of the Association, the attentions and laudations of the great assemblage being specially directed to the eloquent response made by Dr. N. Pinckney to the toast, "The Navy of the United States and its Medical Corps." Other toasts were equally well received, and the interest of the supper was sustained until a late hour in the evening.

FOURTH DAY, MAY 4.—The Association was called to order at the appointed time, 9, A. M., by the President, after which the minutes of the previous sessions were read by the Permanent Secretary, Dr. W. B. Atkinson, of Philadelphia.

Dr. Cox was, on motion, accredited as a delegate to the foreign societies.

Dr. Garrish, of New York, offered the following, which was adopted :

"*Resolved*, That all the members of this Association urge upon the Legislatures of the various States, the great importance of making it compulsory that all marriages, births, and deaths be registered."

Medical Rank in the Navy.—The Naval Committee appointed at the last meeting of the Association having failed to report upon the subject of naval medical rank, it was moved that Surgeons William M. Wood, Ninian Pinckney, and David Harlan, U. S. N., be appointed a committee to report upon the subject at the next meeting of the Association. Adopted.

Various amendments were next brought up and laid upon the table.

The reports of the various sections were then in turn called for, and adopted.

Dr. Holton, of Vt., offered the following, which was unanimously adopted:

"*Whereas*, The author of the Essay, Dr. H. R. Storer, to whom the prize of $100 from this Association was awarded in 1865, refused to receive the amount thus awarded, consequently increasing the resources of the Association to that amount; therefore,

"*Resolved*, That the thanks of this Association are hereby tendered to Dr. H. R. Storer, for this display of liberality."

The Committee on Ethics, appointed to report on the resolutions of the Montgomery, Pa., Medical Society, recommended a reference of the whole matter to the Medical Society of that State.

Dr. Holton offered the following, which was lost:

"*Resolved*, That at the future meetings of this Association there shall be two general sessions, one in the morning and one in the evening, unless otherwise ordered."

Dr. King, of Pittsburgh, offered the following:

"*Resolved*, That this Association, approving of the system of quarantine, proposed by Dr. Marsden, of Canada, as the most effectual means for preventing the introduction of cholera into this country, do earnestly recommend the propriety of its adoption at all our ports of entry, to the favorable consideration of Congress."

The house then on motion, after a little discussion, went into a Committee of the Whole, Dr. Davis being chairman.

Dr. Bell, of Brooklyn, N. Y., was granted the privilege of making a personal explanation of his statements, in reference to cases of cholera on Ward's Island, and although he persisted in his original assertion, the Chair declared that the whole matter was, he presumed, well understood by the Association, there being only a different scientific opinion entertained by two different parties.

The resolution of Dr. King was then taken up, and after much discussion, Dr. J. H. Burge, of Brooklyn, offered the following, which, after eliciting many remarks from Drs. Horton, Storer, Post, Lee, Pinckney, U. S. N., Marsden, and J. Anderson, N. Y., was, on motion, laid on the table. The following is the resolution:

"*Resolved*, That this Association appoint a committee of ten to memorialize Congress to the following effect: That whereas, in the opinion of many eminent physicians, the system of quarantine recommended by Dr. Marsden, of Canada, for protecting our country from Asiatic cholera, would prove effective; therefore,

"*Resolved*, That we earnestly petition the government of the United States to make an immediate and ample appropriation, and take all other necessary measures to test the utility of said system."

The Committee of the Whole then rose and reported accordingly.

The President resumed his seat.

Dr. Cox moved that Dr. J. C. Tucker, of Nevada, be a member by invitation. Adopted.

Dr. Stokes offered the following as the report of the Section on Psychology, which was accepted and referred:

"The Section on Psychology unite in requesting that a committee be appointed to make a report, at the next annual meeting, on Insanity, and ask that Drs. Isaac Ray, of Providence, R. I., Clement A. Walker, Boston, Mass., A. B. Cabaniss, Mississippi, W. S. Chipley, Kentucky, John Fonerden, Maryland, be appointed said committee.

WM. H. STOKES, *Sec.* CLEMENT A. WALKER, *Chairman.*

The report of the Committee of the Whole in reference to the question of quarantine was then adopted by the Association.

Death of Prof. D. L. Magugin, of Iowa.—Dr. Taylor, of Iowa, presented the following:

"WHEREAS, After a long and laborious life devoted to the practice of medical art and promotion of the interests of medical science, Dr. D. L. Magugin, of Iowa, has been called to the final rest of all good men:

"*Resolved*, That the Association, while deeply regretting the loss they have sustained, will ever keep alive the memory of his many virtues and professional worth, and commend the example of his untiring devotion to our common cause.

"*Resolved*, That a copy of these resolutions be furnished his family, with sincere condolence."

Dr. Garrish, of New York, offered the following:

"*Resolved*, That the members of this Association tender their heartfelt thanks to our professional brethren of Baltimore for the liberal, cordial, and satisfactory manner in which they have entertained us."

Dr. D. H. Storer offered his report as delegate to the last meeting of Superintendents of American Institutions for the Insane, and presented the following for adoption:

"*Resolved*, That the Association recommend to the several medical and law schools of the country, the establishment of an independent chair of Medical Jurisprudence, to be filled if possible by teachers who have studied both law and medicine; attendance upon one full course of lectures from whom shall be deemed necessary before the medical degree is conferred.

"*Resolved*, That while this Association regrets that the Association of Superintendents of American Asylums for the Insane has not yet thought fit to unite itself more closely with the representative body of American physicians, it still is of opinion that such union is for their mutual and reciprocal advantage, and that it ought to be effected without further delay.'

On motion, the above was adopted.

After the transaction of business of minor importance, the Association adjourned *sine die*.—[*Boston Medical and Surgical Journal.*

Review of Current Medical Literature.

GENERAL PATHOLOGY AND PATHOL. ANATOMY.

1.—***On Fibrinous Exudation.*** By Prof. BUHL, of Munich. (*Schmidt's Jahrb.*, cxxviii, p. 161; from *Sitzungsber. d. k. bayer. Akad. d. Wiss.*)

We have been accustomed to designate as a "false membrane," as "exudative fibrine," the white or yellowish or pinkish, translucent or opaque, coherent, elastic, or soft friable substance which is found deposited in lamellæ upon a normal membrane, and which can usually be drawn or scraped off with more or less facility. Buhl has demonstrated that the so-called fibrinous exudation in inflammation of membranous surfaces is not identical with the fibrine of the blood; that, consequently, it is not a precipitate, a coagulum, from the exuded liquor sanguinis; that it is not an exudation at all, in the usual sense of pathologists, nor an exuded amorphous but organisable substance. According to B., the "fibrinous exudation" is *either a product of secretion, or a young, vascular, connective tissue:*

(a.) The fibrinous exudation of mucous membrane: Buhl's "epithelial fibrine." B. takes the croup of the air passages as the prototype of this. Contrary to the usual statements, the ciliated epithelium of the mucous membrane is found more or less intact, hence below the pseudo-membrane. The latter arises, firstly, from a peculiar metamorphosis of the epithelial

cells analogous to mucous metamorphosis; and, secondly, by nuclear division, as well as by free endogenous generation of pus corpuscles within those epithelial cells. Accordingly, croup presents manifold analogies to catarrh; while the latter consists in a morbid process of mucous membrane, in which an increased production of mucus and mucous corpuscles takes place, — croup is a process in which, by an altered activity, fibrine is produced instead of mucus, and pus instead of mucous corpuscles. Similar processes occur upon serous membranes also; only an excessive growth of the epithelium takes place at the same time. Moreover, these processes occur upon the endocardium, forming the so-called vegetations; and, finally, in parenchymatous organs whenever they are traversed by canals; in the lungs in ordinary pneumonia; rarely in the smallest biliary ducts; in the kidneys in Bright's disease, in the shape of renal casts.

The so-called *gelatinous exudations* upon serous membranes are such epithelial masses on a large scale, a modification which physically is intermediate between fibrine and mucus.

Like catarrh, the simple and compound (*i. e.*, combined with formation of pus cells) epithelial production of fibrine is associated with swelling of the mucous or serous membrane or interstitial tissue of tubular parenchymata.

(*b.*) The so-called pseudo-membranes of serous membrane: Buhl's "desmoid fibrine," or fibrinoid luxuriance of connective tissue. B.'s prototype of this is the so-called fibrinous pericarditis. This kind of fibrinous exudation is a formation of young connective tissue. It is found particularly upon serous membranes, but also in the interstitial tissue of the parenchyma of organs, more rarely on mucous membranes. Vertical sections through the exudation and the subjacent membrane show three layers: 1. The swelled serous membrane; 2. A vascular layer; and 3. A non-vascular layer of fibrine. The two latter layers are in undistinguishable, continuous connection with the serous connective tissue. The existence, in the recent exudation, of vessels with closed walls which are in communication with the normal vessels, has been indisputably demonstrated by B., by means of artificial injection. The vessels are placed perpendicular to the serous membrane in tufts. Beside the vessels, densely crowded nuclei and cells, mostly spindle-shaped, are lying in the connective tissue. The non-vascular layer is nothing else than the over-growing portion of the exudation, which is characterized not only by its lack of vessels, but also by the small number of nuclei, and on the other hand by more inter-cellular substance. It forms loose meshes and villi, which rarely are covered with an irregularly-shaped epithelium. The serum found with this desmoid fibrine is the real exudation; it is derived in greater part from the new-formed vessels. In some cases this serum deposits fibrine, especially in contact with the atmosphere.

The so-called "hæmorrhagic exudation" is, according to B., a fibrine. the vessels of which have bled. The rupture of the vessels can take place at the ends of the loops, or at their base; the blood mixes with the serum.

Diphtheria (acute necrosis, acute sloughing), according to B., is that form of fibrinoid growth of connective tissue,—whether occurring as a pseudo-membrane or as an infiltration,—in which the luxuriance is so excessive that anæmia, and consequently, rapid disintegration, takes place in it, while the substance turns opaque and yellow (from abundant formation of molecules in the cells and parenchymal fluid), dry and brittle,—in short, that nutrition ceases entirely.

The third form of fibrinoid connective-tissue growth is that in which suppuration takes place, whereby the exudation is rendered brittle, soft, friable, flaky, and dissolved: the fibrinous exudation suppurates, exulcerates. The suppurating false membrane differs in no wise from suppurating connective tissue.

The fibrinous exudation remaining unchanged is finally converted into complete connective tissue; the superficial, into so-called tendinous spots, or into free villi or adhesive tissue; that in the center of organs, into a cicatrix or a stellate cicatrical induration.

2—*On the Anatomical Changes of Chronic Alcoholism.* By E. LANCEREAUX. (*Schmidt's Jahrb.*, cxxix, p. 51; from *Gaz. hebdomadaire.*)

The lesions arising from chronic abuse of alcoholic liquors may be classed in two groups, according as they concern the areolar tissues and belong to the class of adhesive inflammations,—or as they concern the cells of special function, and cause molecular, fatty degeneration.

The alterations of the areolar tissue take place in the interior of organs as well as on their surface, and spare but few organs; but their principal seats are the liver, the brain, the kidneys, and the serous membranes. At first a distinct injection takes place, and numerous nuclei are formed upon the external coat of the smaller vessels; afterward cells and fibres are formed, and the organization progressing, the new-formed tissue contracts more and more, and impresses upon the affected parenchymatous organs a peculiar character. The liver is attacked in its totality, at first increases in size by hyperæmia and new-formation, but soon becomes smaller, hard, and granulated, by the contracting tissue; this state is pathognomonic of chronic alcoholism. In the brain similar changes occur; it becomes smaller, paler, firmer; the convolutions become atrophic; the pia mater usually is infiltrated,

thickened, opaque, covered with whitish spots, or stained by hæmatine. The kidneys sometimes are granulated on their surface, like the liver, firmer and perceptibly atrophic. Similar lesions of the lung were called "chronic pneumonia" by *Magnus Huss*.

Lesions of membranes chiefly occur on the digestive and respiratory tracts. The mucous membrane of the stomach is abundantly vascularized in distinct spots, in the center of which hæmorrhagic coagula or oval erosions are found; afterward it is covered with pigment spots, and indurated, as also the subjacent areolar tissue. A peculiar change sometimes takes place in the walls of vessels which come into direct contact with the resorbed alcohol, especially in those of the portal vein and pulmonary artery; a new-formation is deposited between the different tunics, which, by contracting, may lead even to obliteration of the vessels; hence the frequent occurrence of adhesive pylephlebitis in drunkards.

The second form of alterations consists in the deposition of albuminous or fatty granulations in the parenchyma proper. The cells swell, and are finally destroyed; an enlargement of the organ is the consequence. This process also most frequently attacks the liver; the latter enlarges, chiefly in thickness. The kidneys also enlarge; the cells of the ducts are filled with fat granules, and give the cortical portion a uniform yellow tint. The cells of the gray substance of the brain also undergo the adipose degeneration; so also the cells of the pancreas, of the salivary and gastric glands, of the bronchial ramifications, and even of the seminal ducts. The muscles degenerate in the same manner, especially the heart, which becomes flabby, soft, yellowish; its fibres lose their striation, its cavities dilate.

Important in the diagnosis of chronic alcoholism is the simultaneous occurrence of many or all of the changes enumerated above, accompanied by depositions of fat in the subcutaneous tissue, in the mesentery, omentum, etc.

The two groups of lesions differ in frequency. While the fatty degeneration occurs almost constantly in certain organs, the adhesive inflammations are much rarer. The manner in which

the latter are brought about is not known with certainty; they may be caused by the direct irritating effect of alcohol; the circumstance that the portal vein and liver are most frequently affected seems to favor this view. The adipose degeneration appears to be the consequence of retarded nutrition. The author calls attention to the correspondence of the lesions of this last group with the anatomical changes of old age. Habitual drunkards and the aged both show progressive atrophy of the brain; granular, fatty degeneration of the smaller vessels of the heart, and of most parenchymatous organs; dilation of the pulmonary vesicles; ossification of the costal cartilages; porosity, and fatty change of the bones, etc. So also the course of acute diseases is similar in the drunkard and in the old man; and it may be asserted that alcoholism induces premature old age.

3—*Morbid Anatomy of Pneumonia.* By Dr. A. T. H. WATERS. (*Med. and Surg. Reporter*, April 7, 1866, from *Lancet* [?].)

The vessels involved in pneumonia, according to Dr. Waters, are the capillaries of the pulmonary artery which surround the air-sacs and form the pulmonary plexus. On examining, under a dissecting microscope, a piece of inflamed lung, which has reached the stage of hepatization, it is at once seen that the seat of the exudation is the air-sacs. These cavities are filled with solid matter, and if the preparation has been kept in spirit for some time, moulds of the cavities can be drawn out. As the air-sacs are the seat of the exudation, it is obvious that the exudation must be poured out from their walls; and as they contain no other vessels than those derived from the pulmonary artery, it is the branches of this vessel alone which are involved in the disease.

Some pathologists, in speaking of the morbid anatomy of pneumonia, have described the exudation as taking place, in part, into the interstitial tissue. But they have not, according to Dr. Waters, described accurately what they mean by interstitial tissue; and it is very important that clear notions should exist with reference to this particular point. The lungs are not permeated throughout with areolar tissue, and it exists only in certain points, and in small quantities. The true lung tissue, that which has been known as the parenchyma, consists of the walls of the air-sacs. These walls are firm and strong, but very thin. They consist of yellow elastic tissue, and a basement membrane, enclosing the pulmonary plexus. No areolar tissue is found in these walls; a fact which the morbid condition

produced by pulmonary emphysema fully demonstrates. Although in pneumonia the walls of the air-sacs become somewhat thickened, this is due, the author believes, chiefly to the enlargement of the capillaries which they contain, and only partly to their retaining some of the exudation.

Pneumonia, then, according to the author, consists of an inflammation of the walls of the air-sacs of the lung, and the blood-vessels involved are the branches of the pulmonary artery, which constitute the plexus; and that the capillaries of the bronchial arteries are in no wise implicated, unless there be a concurrent bronchitis, which is in addition to the pneumonia, and not an essential part of it.

PRACTICE OF MEDICINE.

1—*On the Excretion of Uric Acid in Disease.* By Prof. BARTELS, of Kiel. (*Deutches Archiv f. klin. Med.*; I, p. 13. 1865.)

Numerous, exact, and diligent investigations have enabled the author to establish the following propositions:

An increase of the excretion of uric acid beyond the normal quantity, without a simultaneous and proportional increase of urea, is, under all circumstances, the result of incomplete oxydation of the substance of the body,—that is, of a relative insufficiency of respiration. The disproportion between the supply and the demand of oxygen may be based on very different conditions, and is by no means exclusively owing to anatomical changes in the respiratory organs proper, or to disturbed physiological functions of the locomotive apparatus subservient to respiration. The relations of the distribution of the blood, and the condition of the circulating blood may contain just as great impediments to efficient respiration. Disturbance of the respiratory function by any one of these conditions may remain without influence upon the oxydation of the nitrogenous compounds of the body, so long as, during rest, the demand for oxygen remains within ordinary limits; the effect often becomes perceptible only when the demand for oxygen is increased by physical exertion or by fever.

The correctness of this proposition granted, the second proposition would be: *A uric acid diathesis, in the sense of a specific anomaly of organic change, does not exist.* Those cases, from the description of which the uric acid diathesis has been construed, in which a constant discharge of crystalline uric acid by the urine, and the formation of lithic concretions within the urinary passages were observed, do not by any means furnish proof of a more copious production of uric acid in the system. My observations communicated above have even shown in several cases the very opposite. The separation of uric acid from its salts in the urine, the cause of the formation of concretions and gravel, is due to conditions very different from a quantitative increase of uric acid; especially can this deposition be caused by a morbid state of the walls of the urinary passages. * * * *

Now, if it be asked, what gain to practice results from these conclusions arrived at by so much labor? I could answer:

1. Sediments of urates, as well as the deposition of uric acid crystals from the urine, do not by themselves indicate an increased formation of uric acid in the system, and hence do not justify the conclusions we have been accustomed to draw from these phenomena in practice.

2. The proof of a really increased excretion of uric acid always signifies a relative insufficiency of respiration, and may induce the practitioner to search out its cause, unless, perchance, it is obvious.

3. An increased excretion of uric acid, as such alone, may be without injurious effect upon the organism. But the deposition of uric acid from its saline compounds within the urinary passages, may create essential mischief by the formation of concretions. The causes of the separation of crystalline uric acid, before the urine is voided, are various; it may be based on anomalies of digestion, and can then be relieved by a proper diet, and in case of acid fermentation of the gastric contents, by the use of nitro-muriatic acid, inasmuch as this remedy, employed according to the direction of *Budd*, prevents that fermentation. But local diseases of the urinary passages also can give rise to the decomposition of the urine in the pelvis of the kidney and in the bladder, and so to the deposition of uric acid. In these cases I have found the use of oil of turpentine and of balsamic remedies advantageous. In all cases, however, it will be necessary besides to furnish the requisite solvents for the uric acid in the urine, by its dilution, and by the introduction of alkaline carbonates.

2—*On the Tympanitic Sound in Pneumonia.* By Dr. CHR. BAEUMLER, Resid. Physician to the German Hospital in London. (*D. Archiv f. klin. Med.*; I, p. 145. 1865.)

In the course of pneumonia (occurring in previously healthy lungs), a remarkably distinct tympanitic sound is often heard on percussing over the infiltrated part. It is usually explained as owing to altered conditions of vibration, produced by the inflammatory relaxation of the parenchyma, which still contains (or again begins to admit) air; i. e., in the initial stage, and in the stage of resolution. The increasing infiltration would in this case convert the previously tympanitic into a dull sound, which upon resolution, would again pass into a dull tympanitic, and finally into the normal sound. This sequence of signs is sometimes observed, but by no means constant; many cases never give a tympanitic sound, and in others it is absent in the first stage, but appears with great distinctness when the infiltration has reached its height, or lasts throughout the entire course of th
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disease, even while the other signs seem to prove the total absence of air from the affected portion. In such cases, of course, the sound can not be attributed to vibration of the pulmonary parenchyma at all.

Graves and *Stokes* sought to explain the phenomenon by assuming the existence of a pneumothorax. *Hudson* thinks "the explanation might perhaps be found in the facility with which vibrations of the air in the bronchus of the larger divisions might be supposed to be communicated through lung in that condition." (*Dublin Med. Jour.*, 1835, VII, p. 402.)

The author distinguishes three modifications of the sign, viz: 1. A weak tympanitic ring accompanying the dull sound is often detected in the first and last stages. 2. A very sonorous sound, similar to that produced by percussing any space of air surrounded by reflecting walls: it occurs chiefly in pneumonia of the upper lobes, and does not alter its pitch on opening or closing the mouth or nostrils. It differs from the first only in degree. 3. A very sonorous sound, with distinct variation of pitch on opening and closing mouth and nostrils. The two last-named modifications are often accompanied with the sound of the cracked pot.

After relating many cases and post-mortem observations (we will mention the universal absence of pneumothorax, and the frequent remark that the infiltrated portion of lung, small pieces of which would sink in water, still gave the tympanitic sound on percussion, after it was removed from the body), the author sums up the result of his observations. The time of occurrence of this sound is not always the same; the mode in which it is produced is generally decisive in this respect. A weak, tympanitic *nuance* of the dull sound often occurs on the first or second day; the clear, almost amphoric sound, usually showed itself from the third to the eighth day. Over a lower lobe it rarely occurs in the acme of the disease, usually in the stage of resolution. Its duration is likewise variable; sometimes it lasts but two days in entire purity, in other cases it can be heard for eight days, losing the property of altering its pitch sooner, than its tympanitic character.

The circumstances of each individual case must be carefully weighed before a definite conclusion can be drawn from the presence of a tympanitic sound. B. enumerates four possibilities of origin. 1. It is not directly connected with the infiltration of the pulmonary tissue at all, but may be conducted through the infiltrated lung from the great cavities of the abdomen,—the stomach or colon. This takes place almost exclusively in the lower lobes, and the sound coincides in degree of acuteness with that of the stomach or colon respectively. 2. The pulmonary tissue is the source of the tympanitic sound. Whenever lung parenchyma, which normally is kept extended beyond its equilibrium, is enabled to contract, it will yield a tympanitic sound, which takes place in the vicinity of infiltrated parts. A very sonorous tympanitic sound, however, is also produced *by the vibrations of the infiltrated portion itself*, so long as but a minimum of air is contained in the superficial stratum. The tissue has lost its elasticity; it is relaxed, and therefore better able to suffer uniform vibrations, than in its normal state. A clear, full tympanitic sound, therefore, may indicate relative want of air—infiltration—however paradoxical this may appear. 3. When the tympanitic sound is present in a superior lobe, which is infiltrated and thoroughly devoid of air, with the exception of the larger bronchi, it originates in the air columns of these bronchi and the adjacent pharyngeal, oral, and nasal cavities. In this case the sound alters its pitch when the mouth and nostrils of the patient are opened or closed. It is the "broncho-tracheal sound" of *Hudson* or *Williams*. (The same conditions, of course, can be furnished by a neoplasm, or by a large pleuritic exudation, which compresses the entire lung completely.) 4. Finally, it is possible that a tympanitic sound in pneumonia be owing to a cavity (by suppuration or gangrene), or to a pneumothorax. Theoretically, this possibility can not be denied; but the necessary conditions to produce the sound under such circumstances are very rarely observed to occur. (A cavity, if small, must be situated very near to the surface to produce the sound; if large, it will give the same acoustic signs as pneumothorax.

In pneumothorax the tympanitic sound is the exception, not the rule; the change of its locality upon changing the position of the patient is the only sign which gives the tympanitic sound a diagnostic value in pneumothorax.

3—*Trichinæ in Pork raised in the United States.* (*Med. News*, May, 1866.)

A committee was appointed some time since by the Chicago Academy of Sciences to ascertain, first, whether Trichinæ actually exist in the hogs of this country, and in those of the Northwest in particular; and secondly, should they exist, to determine the extent of the danger thereby incurred, and to ascertain the best means of averting it. For the attainment of the first object portions of muscles from 1894 hogs in the different packing houses and butcher shops of Chicago have been examined, and the results presented in tables.

These tables show that the committee "have found trichinæ in the muscles of twenty-eight hogs out of 1894 examined. We may therefore conclude that in the hogs brought to Chicago one in fifty is affected with trichiniasis in a greater or less degree. We must confess our surprise at arriving at this result, which indicates, with little doubt, the startling fact that trichiniasis in pork is even more common in this country than in Germany, where it caused so much suffering and death. For instance, in the city of Brunswick, where a most careful inspection of 19,747 hogs was made in the years 1864-'5, only two were found to contain trichinæ in their muscles, the proportion being 1 to 10,000 against 1 to 50, as before stated, in our country. The comparative immunity from disease which our own people have enjoyed, undoubtedly results from our habit of cooking meat before eating it, while in Germany it is eaten raw by the poorer classes, on account of the high price of fuel."

The committee have found the spinal muscles more liable to be infested with the trichinæ than other muscles.

4—*On Epidemic Diseases.* By Prof. HAMERNIK, of Prague. (*Med.-Chir. Rundschau*, 1866, I, p. 27; from *Casopis lékar. cesky*, 1865.)

To define epidemic diseases is extremely difficult. But few diseases can be included under this definition, the criteria of which are only negative.

They prevail only a short time; before and after this time we find no trace of them. Every epidemy can be circumscribed by a circle, which, however, it can transgress, so that it then gradually extends in a new circle in the same measure as the former one contracts. This process can repeat itself.

In every epidemy there are developed within a certain space, and at a certain time, some like cases of disease—not connected with each other—in different stages and of different intensity.

Finally, it is indispensably essential to the nature of an epidemic that all these disconnected cases of sickness arise from a common source, unknown to us. Thus the origin as well as the progress and decline of an epidemy remain unintelligible to us.

Cholera may stand as a type of all epidemics. It originated on the banks of the Ganges, but spread over nearly all parts of the globe in all climates, among all classes of men, within cities, as well as in the open country; therefore its cause can not be conceived to reside in the condition of the banks of the Ganges, or in the various conditions of life, or in the salubrity, etc.; nor can these be of any importance in relation to the cessation of cholera, for this reason, that they do not change essentially before or after the epidemic.

This negative circumstance is at present so necessary to the characterization of an epidemic, that I can not regard a disease with *known* causes of origin and cessation as an epidemic.

If, e. g., several inmates of a group of houses are attacked by a like disease (although independently) in different intensity and stages of development, we could not pronounce this an epidemic if poisoned milk be the *demonstrable* cause of it, or impure water, or defective sewerage, etc. But, of course, it would have to be shown that these conditions arose only just previously to the epidemic, and that upon their removal the respective cases of sickness ceased to arise.

On the part of physicians epidemics are ascribed to a bad sewerage, etc., but the proof of this remains untold. The pertinent chemical investigations are not made, and physicians are not even able to point out whether these conditions differed before, during, and after the epidemic.

The numerous cases of trichina, which recently occurred in a village near Magdeburg, can not be designated as an epidemic, even if the whole village had died out, for the case is entirely analogous to an arsenical poisoning.

How, then, does an epidemy spread? or how does the number of cases of an epidemic disease multiply?

If the mode of origin of the first and of the last case in an epidemic were known to us, or the mechanism by which an epidemy expires, we would probably also recognize the mechanism of the intermediate cases. Since in an epidemic a contagion can not be spoken of, because the very disconnection of the individual cases is a characteristic of an epidemic, the relation is hereby also indicated in which the intermediate cases stand to one another. Accordingly, the ancient physicians did not explain the epidemy as caused by contagion, correctly recognizing that this would neither explain the beginning nor the end of an epidemic, and that in epidemics the first cases must originate in the same way as the intermediate and the last.

The doctrine—now most widely current—of the contagiousness of epidemic diseases, dates from the sixteenth century, from the Veronese *Girolamo Fracastoro*, who also introduced it into practice; for on his advice the Council was removed from Trieste, where an epidemy had broken out which he pronounced contagious, to Bologna. A good advice, truly, not indeed because the sound could have been infected by the sick, but simply because one can avoid the dangers of an epidemic only by leaving its circuit for the time during which it prevails.

The causes of the outbreak of an epidemic, hitherto unknown, do not cease to exist with the first attack; they persist during its entire course, and there is no reason for explaining the first cases otherwise than the following. The assumption of contagion makes the origin of the first case, as well as the cessation of an epidemic, appear incomprehensible. If, however, the first scattered cases (according to *Fracastoro*) could generate a contagion, why does this disappear at the very time when the number of cases attains its greatest height, whereupon the epidemic declines?

It would, therefore, be the part of the contagionists to demonstrate the existence of contagious matters; we anti-contagionists can not be expected even to disprove such uncertain theories. But we find such proceedings occurring in other scientific fields also; ancient prejudices and inherited theories presume themselves free from all necessity of demonstration, while demanding of the opponent the adduction of the strictest proofs.

The author now describes the first appearance, growth, and decline of an epidemic, and continues: The increase in the number of cases is pretended by the followers of *Fracastoro* to be the consequence of contagion. They confess, it is true, that the first infection may take place in an inscrutable manner; that the disease can not even be propagated by inoculation of the blood; and still they speak of contagion while evidently the conclusion would be more correct, that the first case can not by any means have originated through contagion; that the first and every succeeding case is to be ascribed to one and the same cause, and not to a contagion, since the epidemy else would never cease.

The contagionists only dream of contagion and dissemination, without being particularly clear about it, and it almost seems as if they would use these words for the intimidation of mankind, and as the cloak of ignorance. Regarding the simplest facts they hold the most diverging opinions, and the contradictions that can be proved to them in respect to almost every disease are in part the cause of the discredit into which medical science has fallen with many profound thinkers.

The contagionists also are the cause why the people fear an epidemic so very much; they have occasioned as much mischief as many an epidemic; they have immeasurably injured trade and commerce by the prohibitions based upon their erroneous doctrine.

Two means have been invented by contagionists, under whose assistance epidemics arise and spread—*miasm* and *contagium*.*

* The virus of contagion.

These media are idle theories; not demonstrated by physico-chemical experiments, they are differently interpreted by the contagionists themselves. In general, the emanations of animal or vegetable substances undergoing decomposition are called *miasm* or *malaria*, and considered the effluvia of dead nature; *contagium*, on the other hand, is a product of diseased individuals. Being merely the postulates of an erroneous theory, they do not merit a more detailed exposition.

The generation of epidemics by miasms remains to be proven, since epidemics often prevail in places where there are no miasms, and *vice versa*.

Upon the contagium also they impose an incomprehensible task: It is said to be a product of epidemics. But then it can not be the cause of it, the less so since epidemics begin to decrease just at the time when their product, the contagium, has been accumulated in greatest quantity.

Cholera-miasm is said to have its source on the banks of the Ganges; but as cholera occurs under the most various climatic relations, it can not be deduced from climatic peculiarities of the banks of the Ganges; these have been the same from time immemorial, and yet cholera is a new disease.

Miasmatic diseases are said to be not contagious. Then the theory of miasms, it is true, has no practically injurious consequences.

The contagionists ascribe many epidemics to miasms, and afterwards a contagium is to be developed in the patients; such epidemics are then called miasmatic-contagious.

But what the contagium itself is, how it is transmitted, contagionists have no proofs to show; but they only conjecture that it enters the objects which have been in contact with the patient, permeates them, adheres even to dead bodies or parts of them, migrates into the excretions, etc., and then spreads by mere contact. All pernicious, ungrounded theories!

Now, although the existence of a virus has by no means been proven,—although contagionists are not at all unanimous on contagious diseases, and although no infection can take place by mere *contact*, yet the contagionists have formed a doctrine of portability (*Verschleppung*).

But only sporadic diseases, which are contagious in the sense before mentioned, can be imported, and to this end even contact does not suffice. The importation of epidemics can not be proven.

Quarantines, etc., and similar tortures of man and beast, these are the consequences of the doctrine of *Fracastoro*. The latter, it is true, was repeatedly and thoroughly disproven immediately after its appearance, especially by *Facio*, the founder of the antagonistic school. Nevertheless, *Fracastoro's* doctrine is still the dominant one; it flatters physicians, blinds laymen by its deceptive arguments, and thus attains to great respect.

Lately epidemic cholera was also declared to be a contagious disease, and its contagium is called the cholera-virus. But if a physician speak of a virus, it ought to be demonstrable. Whether we speak of a contagium or a virus in an epidemic, never do we have anything tangible before us. Insane persons are wont sometimes to deem themselves poisoned; in this case also

no poison is demonstrable. *Griesinger's* cholera-virus adheres to the excreted matters, and is transported far and wide by travelers affected with cholerina; and, therefore, all provisions against the importation of the epidemic remain fruitless, because such patients can not be prevented from traveling. According to this, a single atom of such cholerina-patients must infect entire tracts of land.

But the progress of an epidemic of cholera alone disproves this theory; for at the very moment when the greatest amount of these excrementitious matters has been accumulated, the epidemy begins to decline, and all these substances are afterward used in large quantities in agriculture, without injury.

Epidemic diseases do not originate in the patient; and when a contagious, i. e., inoculable disease, under circumstances hitherto unknown, assumes an epidemic extension, it does not spread by virtue of its contagiousness, for such an epidemic also expires.

Epidemics, therefore, can not be imported. The conditions of their development and propagation can not be present in the circuits in which they arise. Whenever these conditions are wanting, there are formed the well-known insulated spots free from the epidemics. Such insulated places do not admit the supposition of having remained free from all contact with the sick, and this overthrows the current theory that epidemics follow the paths of commerce.

Now, if epidemic cases of disease are not the result of contagion, but have a *common* origin, it will be of no use to kill the affected within the epidemic circle; for the cases will continue to occur just so long as the epidemic prevails. For man and beast, therefore, removal to a place free from the epidemic is the only certain preventative.

5—*Cholera.*

From Dr. GEORGE JOHNSON'S excellent "Notes on the Pathology and Treatment of Cholera" (*Brit. Med. Journal*) we extract the paragraph in which he attempts to explain the pathological process of cholera. (*Med. News, March*, 1866.)

What is the pathological explanation of this remarkable train of symptoms? The one great central fact is this, that, *during the stage of collapse, the passage of blood through the lungs from the right to the left side of the heart is, in a greater or less degree, impeded.* Let us now consider the evidence of there being this impediment to the pulmonary circulation; and let us endeavor to ascertain the probable cause and consequences of this obstruction to the flow of blood. Very conclusive evidence as to the existence of impeded pulmonary circulation during life is afforded by the appearances observed in the heart, blood-vessels, and lungs, after death. The *post mortem* appearances within the chest have been described, with

more or less minuteness, by several authors; but by no one, I believe, with so much care and accuracy as by Dr. Parkes. (*Researches on the Pathology and Treatment of the Asiatic or Algide Cholera.*)

In the great majority of cases in which death has occurred during the stage of collapse, the right side of the heart and the pulmonary arteries are filled, and sometimes distended, with blood; while the left cavities of the heart are generally empty, or contain only a small quantity of blood; the auricle being partially and the ventricle completely and firmly contracted. The tissue of the lungs is, in most cases, of pale color, dense in texture, and contains less than the usual amount of blood and air. With respect to the extremely anæmic condition of the lung, when death has occurred during the stage of collapse, there is an entire agreement amongst those authors who have most accurately described the *post-mortem* appearances. There is something surprising, as Briquet and Mignot observe (*Traité Critique et Analytique du Cholera Morbus*), in the contrast between the almost constant occurrence of this extremely anæmic condition of the lung, from which scarcely even a few drops of blood flow when the tissue is cut, and the hyperæmia of most of the other viscera.

There is a most remarkable contrast, too, between this anæmia of the lung when death has occurred during collapse, and the great engorgement of the lungs which is almost invariably found when death occurs in the febrile stage which often follows reaction.

Now, it is evident, from the appearances here described, that during the stage of collapse there is an arrest of blood in the branches of the pulmonary artery *before* it has reached the pulmonary capillaries. The arrest at this point explains the remarkable anæmia of the texture of the lungs, while the hyperæmia of the lungs after reaction is due to engorgement of the pulmonary capillaries. Before I attempt to explain this remarkable arrest of blood, it may be well to allude briefly to certain phenomena in the living but collapsed patient, which afford confirmatory evidence that the pulmonary circulation is greatly impeded. The impeded flow of blood through the lungs, resulting as it must, in a very scanty supply of blood to the arteries, accounts for the character of the pulse in cholera. It accounts, too, for the fact that the pulse has often been observed to increase in power and volume under the influence of venesection, which, by relieving the over-distension of the right cavities of the heart, increases their contractile power. (See Dr. Reid's essay, *On the Effects of Venesection in Renewing and Increasing the Heart's Action under Certain Circumstances.*)

Another appearance which receives explanation from the small stream of blood in the arteries, is that of the shrinking of the integuments, and especially the collapse of the features and the sinking of the eyeballs. The eyes of a patient in deep collapse are often as much sunk as those of a corpse; and the chief cause of this, in the case of both the cholera patient and the corpse, is the more or less complete emptyness of the branches of the ophthalmic artery.

That the arterial stream during collapse is reduced to a minimum, is proved by the fact that arteries of considerable size have been opened during life without the escape of blood. Magendie states (*Leçons sur le Cholera Morbus.* p. 21), that on one occasion he cut across the temporal artery of a patient in collapse and no blood escaped. ("Il ne s'écoula pas une goutte de liquide.") Scott says (*op. cit.*, p. 80), that the temporal artery has often been opened without the escape of blood. He also states that a surgeon, "despairing of other means, cut down upon the *brachial* artery; but so completely had the circulation failed, that no blood flowed."

While, therefore, the small and feeble pulse, the collapse of the features, and the occasional absence of hemorrhage from a wounded artery, are explicable on the supposition that the arteries receive a very scanty supply of blood, these phenomena afford evidence confirmatory of that derived from *post-mortem* appearances, that during the stage of collapse the passage of blood through the lungs is much impeded. It appears, therefore, that during the stage of collapse the blood which is sent into the pulmonary artery is, in great part, arrested in the minute branches of the artery, before it reaches the capillaries of the lungs. What is the cause of this arrest of blood? Some writers have suggested that the blood has been rendered so thick by the loss of serum, that it can not pass through the minute vessels. This theory is scarcely deserving of a moment's consideration. It is entirely at variance with the fact before referred to, that there is no direct relation between collapse and loss of fluid by the bowels; and again, with the fact that the state of collapse passes off, while loss of fluid by purging continues, and while, therefore, the thickening of the blood which, according to this theory, has stopped its passage through the lungs, should be continually increasing. The suddenness with which collapse often occurs, is quite inexplicable by the theory, that thickening of the blood through loss of its water is the cause of that condition. In illustration of the sudden occurrence of collapse, I may refer to the following passages in Sir William Burnett's Report on Cholera in the Black Sea Fleet, in 1854: "The first to be attacked were men already on the list for diarrhœa, several of whom fell into a state of collapse one after the other; but, about the same time, robust, healthy men, who had fallen suddenly down in a state of collapse, began to be brought in from various parts of the ship (the *Britannia*), even from the yard, where they were seized while reefing sails." And the surgeon on board the *Albion* reported, that "the attacks in many instances were so sudden, that many fell as if they had drank the concentrated poison of the upas-tree." The supporters of this theory of blood-thickening as the cause of collapse would have us believe that, in the course of a few minutes, the blood of these robust men had become so thickened by loss of water as to be incapable of transmission through the minute vessels of the lungs. I shall hereafter show that the thickening of the blood is a *consequence* and not a *cause* of the arrest of blood in the pulmonary arteries.

I believe the true explanation of the arrest of blood in the lungs to be this: The blood contains a poison whose irritant action upon the muscular tissue

is shown by the painful cramps which it occasions; the blood thus poisoned excites contraction of the muscular walls of the minute pulmonary arteries, the effect of which is to diminish, and in fatal cases entirely to arrest, the flow of blood through the lungs.

We have seen that the condition of the lungs after death, during collapse, affords conclusive evidence that the arrest of the blood occurs, not in the capillaries, but in the branches of the pulmonary artery, before the capillaries are reached by the blood. We know that the walls of the arteries are muscular, and that they have the power of contracting upon their contents under the influence of a stimulus, such as cold, electricity, or mechanical irritation. I suppose that no physiologist at the present day would deny that spasm of the arteries is as real a fact as spasm of the muscles.

Many experiments and observations prove, that contraction of the muscular walls of the arteries has great influence on the passage of their contents. For instance, it is a well-known fact, that the tissues of an animal immediately after death can not, without a force which endangers the integrity of the vessels, be injected with any of the fluids which are commonly used for that purpose. The coats of the arteries, so long as their vital tonicity remains, contract upon their strange contents, and impede the passage of the injection into the capillaries.

Nearly a century ago, Hales (*Statical Essays*, 1769) performed some ingenious experiments to demonstrate the power which arteries possess to control the flow of various liquids through them. His experiments were performed on animals recently killed; and he found, that while warm water passed very readily through the arteries, cold water, decoction of bark, and brandy, passed much more slowly.

Some experiments performed by Blake (*Edinburgh Medical and Surgical Journal*, vols. liii., liv., and lvi.) on living animals bear upon this question. He found that a concentrated solution of a salt and soda, when injected into the jugular vein of a dog, killed the animal in less than a minute. On examination after death, the right side of the heart was found greatly distended, while the left only contained a little black blood. A few grains of nitrate of silver in solution destroyed life in precisely the same way. The passage of blood through the lungs is arrested, and the animal dies with the right cavities of the heart distended, while the left cavities are nearly empty.

The sudden arrest of the flow of blood through the lungs, in consequence of the accidental admission of air through a wounded vein in the neck or axilla, has some relation to this subject. The entrance of air has usually been indicated by a peculiar gurgling sound in the wound, and the symptoms which rapidly follow are thus described. (Dr. John Reid's *Physiological, Anatomical, and Pathological Researches*, p. 553.) "Speedy occurrence of syncope, which is either preceded by a cry, with the expression, 'I die,' 'I am dead,' 'I suffocate,' or by anxiety and tremblings. Or, without any such precursors, the syncope rapidly reaches such a degree that all consciousness is lost, and the patient falls down; cold sweat breaks

out on the forehead; and, in a quarter of an hour, sometimes sooner, sometimes later, he is dead." After death, both in the human subject and in animals that have been the subjects of experiments, the right side of the heart is found to be much distended with frothy blood; and the same mixture of air and blood is usually found in the pulmonary arteries. The left side of the heart is usually empty; but a small quantity of frothy blood is occasionally found in the left cavities and in the aorta. The cause of death in these cases is the distension of the right side of the heart, which results from the impeded transit of frothy blood through the vessels of the lungs. Mr. Erichson found, by experiments on a dog recently killed, that beaten bullock's blood, mixed with air, required nearly twice the pressure to drive it through the pulmonary vessels, that would suffice to drive unmixed blood through the lungs. (*Edinburgh Medical and Surgical Journal*, vol. lxi.)

I have referred to these experiments in illustration of the general principles: 1, that the movement of blood through the lungs may be quickly arrested by the addition of some foreign ingredient to the blood; and 2, that this arrest is probably due to the power which the arteries possess to contract upon their contents.

The proof that the blood is arrested during the stage of collapse in cholera is, as before stated, partly the anatomical condition of the lungs after death, partly the symptoms observed during life.

About two years since, I had under my care in the hospital a woman (S. B.), who was suffering from dropsy, the result of heart disease of long standing. On going through the ward one afternoon, I saw her sitting up in bed, as well as usual. In less than five minutes after I left her to go into another ward, I was sent for in consequence of her having become suddenly worse. I found her gasping for breath, cold and pulseless, with a blue and shrunken appearance of the features, exactly resembling the collapse of cholera. I at first thought that she would die in a few minutes; but she rallied in some degree, and lived forty-eight hours, during which time her dropsical legs became rapidly gangrenous. We found, as I had predicted, that besides old standing disease of the mitral valve, there were firm fibrinous coagula in the branches of the pulmonary artery. The sudden obstruction of the pulmonary arteries by fibrine caused a state of collapse exactly resembling that of cholera. (Hospital Case-book, vol. xix. p. 59.)

The most interesting and conclusive evidence that arrest of blood in the lungs is the true key to the pathology of choleraic collapse, is to be found in the simple yet complete explanation which it affords of all the most striking chemical phenomena of the disease,—the imperfect aëration of the blood, the fall of temperature, the dark and thickened appearance of the blood, and the suppression of the bile and urine.

Chemical consequences of the obstructed flow of blood through the pulmonary arteries. It is obvious that the stream of blood from the pulmonary capillaries to the left side of the heart is the channel by which the supply of oxygen is introduced into the system. One necessary consequence, then, of

a great diminution in the volume of blood transmitted to the left side of the heart, must be that the supply of oxygen is lessened in a corresponding degree. This position probably will not be disputed by any one who will give the subject a moment's consideration. Nor again, can it be denied or doubted, that certain results must of necessity follow this limited supply of oxygen.

The combustion of those constituents of the blood which are normally subjected to the action of oxygen will be diminished in proportion to the deficiency of that gas; and thence follows simultaneously and of necessity a reduction of temperature, and so scanty a formation of urinary and biliary constituents, that, while the state of collapse continues, the functions of the kidneys and liver are virtually suspended. The blood at the same time has that black, thick, treacly appearance, which is not peculiar to the collapse of cholera, but which is common to it with all diseases which are attended with a defective aëration of the blood. The blood has this character during a cold stage of a severe ague fit. Dr. McIntosh, and others, who have bled patients during the cold stage of ague, describe the blood as flowing from the arm at first in a slowly trickling stream, being of a dark color, and not coagulable. (See McIntosh's *Practice of Physic*, vol. i. p. 86 *et seq.*) Dr. Dundas Thomson, who published an elaborate paper on the Chemistry of the Blood in Cholera (*Med.-Chir. Trans.*, vol. xxxii, p. 67), states that one specimen of blood from a "patient who labored under an affection of the mucous membrane of the air-tubes," contained a greater excess of solids, in proportion to water, than he had found even in cholera blood.

The blood in cholera is black and thick only during the stage of collapse; in other words, during the stage of pulmonary obstruction and defective aëration. This state of the blood bears no relation to the loss of water; it comes on when the loss of water has been very trifling; it passes off rapidly, while loss of water by purging continues unchecked. It is simply a result of defective aëration.

The constituents of bile and urine and carbonic acid are all results of oxydation; none of them can be formed without a large supply of oxygen. Suppression of bile and urine during the stage of collapse is a necessary consequence of the limited supply of oxygen which results from the obstruction of the lungs. The amount of carbonic acid expired during collapse is also much diminished. (See, on this point particularly, Twining's *Clinical Illustrations*, etc., 2d ed. p. 15.) One fact confirmatory of the view that the very scanty formation of bile, urine, and carbonic acid, during collapse is a result of the diminished supply of oxygen, is, that *the secretion of milk is apparently undiminished.* I have myself observed this fact, and it has been mentioned by several authors. Thus Magendie states, that one of his patients having been delivered of a child a few days before she was seized with cholera, the secretion of milk continued as abundant through a first and a second collapse, which ended finally, that it was necessary to empty the breasts, in order to relieve the pain which their distention occasioned.

Now, if the other secretions are suppressed on account of the deficiency of water in the blood, why does the secretion of milk continue? Magendie says, because the blood reaches the breasts, and supplies the materials for their secretion, on account of those glands being nearer to the heart than the liver and kidneys, which being at a greater distance, do not receive the supply of blood necessary for the discharge of their functions! My explanation of this remarkable and instructive fact is simply this. The chief constituents of milk—casein, sugar, oil, and water—may be obtained from the blood without the addition of oxygen. The secretion of milk, therefore, continues during the stage of collapse; while the highly oxygenized secretions are suspended, their essential constituents being formed only in very minute quantities during that stage, on account of the defective supply of oxygen.

On the subject of Treatment, *Braithwaite's Retrospect* (Jan., 1866), gives the following brief synopsis, a little apodictic in style, but representing, we believe, the prevailing opinions among the best informed:

The poison of cholera acts through the blood chiefly on the ganglionic nervous system, at first probably rousing it up to violent action, but followed very rapidly by its paralysis. Thus, at first, we have spasms of the organic muscles of circulation, respiration, voice, of the intestinal canal, and many of the voluntary muscles. The consequence is, that the blood is sent into all the great central veins, and can not get round to the left side of the heart, on account of the same spasm or contraction of the branches of the pulmonary artery. In short, the circulation is stopped, or nearly so. Then follows the real collapse, when spasm relaxes; but it is the relaxation of death, unless the vital power has enough energy to begin again. On the first attack of real Asiatic cholera, therefore, do not stimulate; do not interfere with either vomiting or purging*; but use the utmost diligence in giving *chloroform*, both by inhalation and internally. Teach the nurse how to do it, as you can not be everywhere, and can not be both doctor and nurse. Relax the cramps and spasms by chloroform; and, if necessary, carefully stimulate by ammonia or champagne, but *not until the spasm is overcome*. The circulation will then be resumed, and not be followed by the dangerous reaction, which formerly killed so many patients. But avoid all stimulants if you can, and let the reaction come slowly, and if you feel compelled to stimulate, let it be by spt. am. co., champagne, or other mild wine. Give copious draughts of cold water, and even encourage vomiting. Give copious saline effervescing draughts, if agreeable.

*The premonitory purging of cholera may be cautiously treated in the way we treat English cholera; but, if it prove to be the real Asiatic cholera, we think that all astringents ought to be at once given up.

Do not give opium at all; let the purging alone. It is supposed that it does more good than harm by *eliminating* the poison; and some even increase the purging by giving castor oil. Encourage vomiting by large quantities of cold water, with a little mustard in it, if necessary. The muscles of vomiting are so energetic that the water will often be ejected with violence; but keep repeating the cold water, which is always greedily swallowed. There seems to be no specific power in calomel or any other medicine. Apply external cold or warmth, as may be most agreeable. The best way to apply warmth is to wrap the patient up in a blanket wrung out of hot water, and over this hot-wet blanket place three or four other blankets. The hot blanket can at any time be renewed, and if you like, you can, during its removal, wipe the body over with a towel or sponge wrung out of *cold* water. The skin will be powerfully stimulated by this treatment to draw the blood back from the central veins, as soon as the contraction of the pulmonary capillaries will allow the blood to get round to the left side of the heart. Another method is to place the patient in a hot hip-bath with his feet in a foot-bath, the hot water to have about a pound of flour of mustard in it, and to be kept hot by adding hot water as required till reaction sets in, and the cramps and spasm relax. We have said nothing about bleeding, because, although we think that the practice was correct as laid down by the several writers mentioned, yet we have in chloroform and other antispasmodics all the good effects of bleeding without danger. And as to stimulants, they are generally injurious: a combination of compound spirit of ammonia with chloric ether is one of the safest and best stimulants we possess; and champagne and other sparkling wines are now so cheap and good that we should recommend them before all others. (Dr. Braithwaite, p. 265.)

The Philadelphia *Medical and Surgical Reporter* thus announces its position on the contagion and quarantine question (Feb. 17, 1866, p. 133), viz:

Some letters which we have received indicate that we have been strangely misunderstood in certain quarters. Yet we thought that nothing could be more plain than our position in regard to cholera and quarantine, and the language in which that position was explained.

We reiterate what we have said in former articles:

1. We do not believe that cholera belongs to the specifically *contagious* diseases, such as small-pox, etc.

2. But we *do* believe that there is the fullest evidence that cholera is communicated and propagated, under favorable conditions, by the dejections and evacuations of the patients.

3. That the practical question is, not whether cholera is specifically contagious, but whether the medical profession is willing to assume the responsibility of the whole and total abolition of quarantine?

The last question is the one which we would like to see discussed by those who erroneously believe us advocates of the contagiousness of cholera.

Scientific Department.

UNDER THE DIRECTION OF BENJAMIN F. SHUMARD, M. D., ST. LOUIS.

METEOROLOGY OF APRIL AND MAY, 1866.

BY GEORGE ENGELMANN, M. D., ST. LOUIS.

	APRIL.		MAY.	
	1866.	Average.	1866.	Average.
Barometrical Pressure—				
Mean	29″.467	29″.485	29″.441	29″.459
Highest	29″.918	30″.091	29″.670	29″.940
date	6th.	8th, 1865.	21st.	5th, 1842.
Lowest	29″.152	28″.640	28″.842	28″.611
date	22d.	28th, 1846.	27th.	5th, 1861.
Temperature—				
Mean	59°.8	56°.1	64°.2	66°.3
Highest	91°.0	93°.0	87°.5	97°.0
date	27th.	18th, 1855.	19th.	14th, 1836.
Lowest	33°.0	18°.0	41°.0	29°.0
date	8th.	6th, 1857.	2d.	2d, 1851.
Evaporation	7°.9	7°.2	8°.5	8°.2
Relative Humidity	56.1	57°.0	56.2	59.6
Rain—				
Quantity	1″.56	3″.96	2″24	4″.94
Days on which it fell	6	9	7	9
Thunder-storms	3	5	2	7
Principal Winds	SE.	W. & SE.	Se., Nw., Sw., Ne.	SE., then W.
Cloudiness	4.3	4.8	4.6	4.4
Fair days	13	11	13	11
Variable days	14	15	15	17
Days without sunshine	3	4	3	3
Stage of River—				
Mean, feet	20¼	18	20¼	20
Extremes, feet	12½—26¾	8—31	14—24½	9.33

The atmospheric pressure has been, in the months of April and May, as the table shows, rather lower than the average, nor were the extremes as great as they often are; the temperature, in March lower than the average, was in April much higher, and in May considerably lower again. But the most remarkable feature of the climatic appearance of the last two months was their unusual dryness. The quantity of rain that fell in the two months together did not amount to the average quantity for

April alone, and was much less than the average quantity for May. I have, in twenty-eight years, not observed as little rain in May as in this year. The relative humidity was correspondingly lower, and the number of fair days greater. The effect of this cool and dry weather on the public health has been most favorable. Though spring intermittents prevailed to a considerable extent, they were, as is always the case, owing more to the effects of the previous autumnal endemic influences, together with the first hot and changeable weather of spring, than to any peculiar effect of the latter. Spring never brings us intermittents, to any extent, unless a sickly fall precedes it. The city is, thus far, remarkably free of the diseases incident to our climate, and which are the effects of a wet and hot spring; such as bilious diarrhœas, dysenteries, and cholera morbus, which even at the late date of this writing (the end of June), have not yet made their appearance to any extent.

COMPARATIVE METEROLOGY OF THE SPRINGS AT ST. LOUIS.

	1866.	1865.	Average.
Barometrical Pressure—			
Mean	29″.518	29″.498	29″.494
Highest	30″.078	30″.091	30″.305
date	March 8.	April 8.	March 5, 1848.
Lowest	28″.842	28″.928	28″.516
date	May 27.	March 21.	Mch. 28, 1860.
Temperature—			
Mean	55°.3	55°.7	55°.6
Highest	91°.0	88°.0	97°.0
date	April 27.	May 20.	May 14, 1836.
Lowest	13.5	10°.5	9°.0
date	March 17.	March 10.	March 3, 1848.
Evaporation	6°.9	6.°2	6°.9
Relative humidity	59.4	62.8	59.1
Rain—			
Quantity	6″.60	17″.58	12″.71
Days on which it fell	20	36	26
Thunder-storms	6	13	15
Principal Winds	SE., next NW.	SE., next NW.	SE. & W.
Cloudiness	4.9	4.4	4.6
Fair days	29	35	34
Variable days	49	45	47
Days without sunshine	14	12	11
Stage of the River—			
Mean, feet	19 1-5	17½	17½
Extremes, feet	13—26¾	10—25	6—33

The atmospheric condition of the spring (March, April, and May) was marked by its great dryness, the quantity of rain having been about one-half of the average for the season, and not much more than one-third of what it amounted to last year. We had fewer thunder-storms than usual, but the number of fair days was less; the proportion of cloudiness was greater than commonly, and greater even than last year, with all its rain. Most other atmospheric conditions were about normal. The effects on the health of the city have been stated above. I may add here, that thus far, if no excessive rain-storms supervene in the next five or six weeks, we have the prospect of a very healthy autumnal season. The fear of epidemic cholera has, I suppose, by this time, quite disappeared from the minds of the public. There is no probability, not the least, of its appearing among us this season. The course this epidemic may take next year can not be prognosticated by us at present.

DEODORIZATION AND DISINFECTION.

Dr. J. H. Baker, in his prize essay on "Deodorization and Disinfection," sums up the result of several series of experiments in the following propositions:

"1. For the sick-room, free ventilation, when it can be secured, together with an even temperature, is all that can be required.

"2. For rapid deodorization and disinfection, chlorine is the most effective agent known.

"3. For steady and continuous effect ozone is the best agent known.

"4. In the absence of ozone, iodine exposed, in the solid form, to the air is the best.

"5. For the deodorization and disinfection of fluid and semi-fluid substances undergoing decomposition, iodine is best (employed in the form of tincture).

"6. For the deodorization and disinfection of solid bodies that can not be destroyed, a mixture of powdered chloride of zinc, or powdered sulphate of zinc, with sawdust, is best. After this, a mixture of carbolic acid and sawdust ranks next in order; and following on that, wood-ashes.

"7. For the deodorization and disinfection of infected articles of clothing, etc., exposure to heat at 212° Fahr. is the only true method.

"8. For the deodorization and disinfection of substances that may be destroyed, heat to destruction is the true method."—[Chem. News.

ATMOSPHERIC ELECTRICITY, TEMPERATURE, AND HUMIDITY.

Observations made at St. Louis, Missouri, based on daily observations at 6, 9, 12, 3, 6, and 9 o'clock, from morning till night.

BY A. WISLIZENUS, M. D., ST. LOUIS.

I.—*Monthly Mean of Positive Atmospheric Electricity.*

APRIL.

YEAR	6 A. M.	9 A. M.	12 M.	3 P. M.	6 P. M.	9 P. M.	MEAN OF MONTH	NO. OF THUNDER STORMS.	PREVAILING WINDS.
1861...	10.8	11.1	7.1	7.0	7.7	9.3	8.8	2	S. & W.
1862...	10.7	13.7	10.6	9.1	9.4	10.1	10.6	6	SE. NW. & NE.
1863...	10.0	9.9	10.0	6.2	8.0	8.6	8.8	2	SE. & NW.
1864...	9.1	11.6	6.8	7.5	7.8	8.0	8.5	3	NW. & SE.
1865...	3.8	4.8	2.7	3.3	2.7	2.6	3.3	4	W. & SE.
1866...	2.4	3.3	3.3	1.9	0.6	1.3	2.1	3	SE.
Mean.	7.8	9.1	6.7	5.8	6.0	6.6	7.0	.....	.....

MAY.

1861...	9.3	9.5	8.1	6.3	6.1	7.2	7.8	5	W. & SE.
1862...	8.0	8.3	8.4	6.2	8.0	6.3	7.5	5	SE. & NE.
1863...	6.9	4.5	4.6	3.0	5.2	3.7	4.7	5	SE., S., N. & NW.
1864...	5.1	6.9	5.8	4.8	4.5	3.8	5.1	1	N.
1865...	3.7	3.4	2.2	1.4	1.5	2.0	2.4	7	SE., N. & SW.
1866...	4.3	3.3	2.1	3.3	4.0	2.6	3.3	2	NE. & SW.
Mean.	6.2	6.0	5.2	4.2	4.9	4 3	5.1	.....	.....

2.—*Monthly Mean of Temperature, Fahrenheit.*

APRIL.

YEAR.	6 A. M.	9 A. M.	12 M.	3 P. M.	6 P. M.	9 P. M	MEAN OF MONTH.
1861	49.6	56.5	62.7	65.1	59.8	55.1	58.1
1862	46.5	53.3	59.5	60.8	57.6	52.3	55.0
1863	47.3	55.7	62.9	64.4	60.0	54.4	57.4
1864	45.0	50.8	55.3	56.6	54.1	49.5	51.4
1865	48.7	55.0	60.8	62.9	59.2	54.5	56.8
1866	52.1	59.0	66.4	68.3	63.0	58.5	61.2
Mean	48.2	55.0	61.3	63.0	58.9	54.0	56.6

MAY.

1861	54.7	62.9	68.7	70.8	67.1	60.2	64.1
1862	58.5	69.2	75.4	77.6	72.2	65.5	69.7
1863	56.1	66.1	70.6	70.8	67.8	61.4	65.5
1864	58.2	69.1	74.4	76.8	73.3	65.3	69.4
1865	58.9	68.4	73.2	75.7	71.7	64.7	68.8
1866	56.0	65.6	71.4	73.2	69.7	61.8	66.3
Mean	57.1	66.9	72.3	74.1	70.3	63.1	67.3

3.—*Monthly Mean of Relative Humidity.*

APRIL.

YEAR.	6 A. M.	9 A. M.	12 M.	3 P. M.	6 P. M.	9 P. M.	MEAN OF MONTH.
1861	77.4	62.4	53.2	51.7	57.3	67.2	61.5
1862	82.0	68.3	57.3	55.9	63.0	75.8	67.0
1863	76.5	57.8	46.9	43.9	53.0	65.4	57.2
1864	85.1	72.7	62.2	57.6	63.2	77.8	69.8
1865	80.4	66.3	57.3	56.3	63.5	77.2	66.8
1866	76.3	61.5	51.2	48.7	58.7	67.5	60.6
Mean	79.6	64.8	54.7	52.3	59.8	71.8	63.8

MAY.

1861	83.5	67.5	59.0	54.7	58.4	74.5	66.3
1862	77.7	54.9	46.0	43.1	54.4	68.0	57.3
1863	81.8	56.8	46.6	46.9	53.7	70.8	59.4
1864	78.3	55.5	49.4	43.6	46.6	65.1	56.4
1865	80.9	60.9	53.3	47.7	55.3	74.3	62.1
1866	76.0	57.6	50.5	47.8	54.9	71.2	59.7
Mean	79.7	58.9	50.8	47.3	53.9	70.6	60.2

Editorial Department.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Rhinoscopy and Laryngoscopy; their value in Practical Medicine. By Dr. Friederich Semelder. (Translated from the German by Edward T. Caswell, M. D.) Published by William Wood & Co., New York.

All means to aid us in physical diagnosis are eagerly sought after, and the appearance of this book is gladly welcomed. For a student who is just beginning the arts of Rhinoscopy and Laryngoscopy, this book of Semelder is particularly interesting. The history and literature, the anatomy, the physiology, the pathology, and the therapeutics, are quite elaborately entered into, and admirably developed. The chapter on the practical application of Rhinoscopy is particularly interesting, for as Semelder justly says, "the treatment of the diseases of the ear forms, as is well known, one of the weak points in the healing art, in consequence of the concealed position of the organ, which renders it only in a limited degree accessible for diagnosis as well as for therapeutic applications."

By means of the Rhinoscope, all this is improved; and the eustachian tube — that *pons asinorum* of aural surgery — is brought under direct notice, and we are enabled to treat the root of the evil by direct contact, instead of the somewhat difficult and always unsatisfactory method of catheterization through the mouth or nasal cavity. Deafness, the result of mucous infiltrations, œdemas, polypi, protuberance of the posterior ends of the turbinated bones, *et id omne genus*, is now easily understood, and consequently overcome. Numerous pathological observations are now made upon the living naso-pharyngeal cavity, which formerly were only revealed by *post-mortem* examinations, and a great boon has thus been rendered to afflicted *ozœnæd* patients, heretofore almost incurable. To such men as Czermak, Stŏrk, Semelder, and Voltolini, the profession owes its unbounded gratitude, for the patient workings and the practical results of their labors. They are the real advancers of our science who build the

imperishable monuments of greatness, and who deduce results from absolute law and experimental research, and who form theories afterwards.

The second portion of the work is devoted to Laryngoscopy, and is divided into special chapters, as that on Rhinoscopy. To say that it is worthy of perusal is not enough, for no accomplished physician or surgeon can now presume to treat the throat scientifically without the Laryngoscope; therefore the knowledge of its use is indispensable, and Semelder's or Mackenzie's book thoroughly teaches it. M. A. P.

RECENT ADVANCES IN OPHTHALMIC SCIENCE. THE BOYLSTON PRIZE ESSAY FOR 1865. By HENRY W. WILLIAMS, M. D., Ophthalmic Surgeon to City Hospital, Boston; Lecturer on Ophthalmic Surgery in Harvard University, etc., etc. Boston: Ticknor & Fields, publishers. From Crawford's, St. Louis.

This little book is gotten up in good style, and to the Ophthalmologist is quite interesting; it is a concise and clear exposition of the late discoveries of Graefe, Donders, Helmholz, and other great Opthhalmists in Europe. Without presenting anything very original, the first chapter on the Ophthalmoscope is admirably put, as well as the second, on the principle and construction of optometers. The most instructive chapters are those of the anomalies of refraction, viz., Myopia, Hypermetropia, and Asthenopia. Presbyopia is now known to be an anomaly of accommodation, and not as formerly considered, the opposite of myopia, as the refractive power for distant objects remains undiminished in presbyopia, parallel rays being still brought to a focus on the retina; the change consisting of a recession of the near point from the eye, and a necessary limitation of the range of accommodation, occasioned by an increase of hardness in the crystalline lens, so that its form can no longer be readily changed, and its convexity increased by action of the ciliary muscle.

The chapter on the "Function of Accommodation," a *résumé* of the labors of Donders, of Utrecht, is of interest; the scientific and mathematical deductions, reduced to great exactness, are of practical importance, and materially assist us in our observations.

Ophthalmology is now being pursued as a speciality all over the country, and such men as Williams are doing much to elevate the oculist from the wandering charlatan, who travels from place to place, defrauding the public by false pretenses and promises. The period is rapidly advancing when Ophthalmology, as well as other specialities, will be looked upon as necessary branches of education for a medical man. In Europe it is already so regarded, and persistent efforts are being made to have it so done in this country. The Bolyston Prize Essay is, therefore, cordially greeted as another lever toward the elevation of the standard of medical education in the United States. M. A. P.

CLINICAL NOTES ON UTERINE SURGERY, WITH SPECIAL REFERENCE TO THE MANAGEMENT OF THE STERILE CONDITION. By I. MARION SIMS, M. D.

The republication of this work in this country, it having been published in London, is a desideratum to such practitioners as pursue Gynæcology as a study. Many of the views therein published by our distinguished countryman have been presented to the profession through the columns of the "London Lancet," and have met with great opposition, both in Great Britain and on the continent. Nevertheless, this by no means proves that they are not extremely valuable. We believe that Dr. Sims has made great improvements in uterine surgery and therapeutics, and we therefore recommend his book very highly. Most of his plans of treatment are novel and ingenious, and based upon the soundest physiological and pathological data. While we commend his chapters on Menorrhagia, Dysmenorrhœa, Anteversion, and

Retroversion, we most emphatically condemn as unworthy of the dignity of the profession, the duties recommended to the medical man in his chapter on artificial fertilization. In a word, the scientific physician is called upon to perform the disgusting duties of being near, almost present, when the sexual congress takes place, with instruments and appliances ready to inject the semen into the uterus after it has been deposited in the vagina! Affairs of State may sometimes require that issue should take place, but in this country nothing ought to make a high-minded surgeon or physician stoop to Sims' "*artificial fertilization.*" With these exceptions, the work before us is undoubtedly a triumph of genius and patient skill. M. A. P.

A TREATISE ON MILITARY SURGERY AND HYGIENE. By FRANK HASTINGS HAMILTON, M. D., late Lieutenant-Colonel, Medical Inspector U. S. A.; Professor of Military Surgery, etc., etc. Illustrated with 127 engravings. New York: Baillière Brothers. 1865. 8vo. pp. 648.

Dr. Hamilton's new work is one of the good fruits of the late war. A comparison of this volume with the short treatise on Military Surgery by the same author, published at the commencement of the war, illustrates the progress of military art in general during the four years which elapsed.

An introductory chapter, on the meaning and proper limits of military surgery, points out in what particular the education of the military surgeon must be enlarged beyond that of the civil practitioner. "It is the especial province of military and naval surgery" to suggest "inferior alternatives for exceptional cases," "instructing the pupil how, by a multitude of extemporaneous expedients, he may succor the wounded and relieve the sick when the usual resources fail or are not at hand." The same chapter bears honorable testimony to the good conduct, bravery, and faithful performance of duty, of the medical officers during the late war, and discusses the question of rank with an able and acute pen, justly censuring the illiberal conduct of the Government

in this respect. One chapter is devoted to the examination of the recruit. Chapters iii. to viii. treat of the general hygiene and general hygienic management of troops, the measures necessary to insure health in bivouacs, barracks, hospitals, etc., and of the conveyance of the sick and wounded. Then follows the strictly surgical part of the work (a portion of which, on hospital gangrene, is from the hand of the author's son, Dr. F. H. Hamilton, jr.), concluding with a chapter on the employment of anæsthetics in major operations, after gun-shot injuries.

To the readers of this *Journal* it may be interesting and satisfactory to hear of the very high terms in which the author speaks of and describes Dr. Hodgen's Wire Suspension Splint, for gun-shot fractures of the thigh, described and figured in this *Journal* (Vol. I., No. 1, new series), which, in his opinion, "is much superior to Smith's Anterior Splint. It has been placed among the army hospital supplies, and is at the present time in great favor with our army surgeons."

We regret being compelled by want of space to desist from a more detailed review of the book before us, which richly deserves our attention and praise. It is thoroughly original; written in a most attractive style, and answers its purpose admirably.

G. B.

DIARRHŒA AND CHOLERA; THEIR ORIGIN, PROXIMATE CAUSE, AND CURE, THROUGH THE AGENCY OF THE NERVOUS SYSTEM, BY MEANS OF ICE. By JOHN CHAPMAN, M. D., M. R. C. P., M. R. C. S. Reprinted, with additions, from *Med. Times and Gazette*. Philadelphia. 1866. 12mo. pp. 57.

Dr. Chapman's treatment of various diseases by means of "spinal bags," by heat or cold applied to the spinal column—*vaso-motor therapeutics*—is here applied to diarrhœa and cholera. According to our author, both diarrhœa and cholera are directly owing to a hyperæmic condition "of those nervous centers which preside over the bowels, and notably, therefore, of the superior and inferior mesenteric plexuses." The treatment

consists in exercising "a sedative influence over those centers by lessening the amount of blood in them," which is effected "by the application of the spinal ice-bag along the lumbar and lower dorsal regions;" in all cases continued until the diarrhœa is subdued, and repeated two or three times daily, until the patient's strength is recovered.

The primary causes of diarrhœa the author conceives to be: heat, motion, nervous irritation (electricity), and (mental) emotion. Cholera is represented as only an intensified diarrhœa! For want of time, the author has left his theory of cholera undeveloped, giving but a sketch; but, as it stands, it fails to explain the epidemicity of the disease; for according to it, the epidemic should always strictly coincide with the hottest season and the hottest years. G. B.

USE OF PRESSURE IN THE TREATMENT OF GONORRHŒAL AND PURULENT OPHTHALMIA. By JOSEPH S. HILDRETH, U. S. V. 1865.

The author of this small *brochure* was in charge of the Desmarres (U. S. A.) Eye and Ear Hospital, Chicago, Ill., and the report, which was read before the American Ophthalmological Society, gives a résumé of the cases treated, the result, and the method employed.

His statement embraces "eyes treated with pressure, 15; without, 17. Of the former 14 were saved, and one lost; of the latter 7 were saved and 10 lost." The general treatment of the cases was the same, except in the use of pressure, and the favorable results obtained seem justly to be due to the addition. He advises "firm, hard, continued pressure upon all parts of the contents of the orbit, especially the anterior. This is effected by packing the orbit (the eye being closed) with charpie or picked lint, so that all parts within the orbit, the anterior hemisphere of the globe, and especially the conjunctiva shall be acted on. Compression is thus made by a bandage, or better by a rubber band, two inches in width, passing around the head. The pressure should be slowly and regularly increased until the pain in the parts affected is greatly diminished or controlled."

Pressure was not alone used in the treatment of his cases, but a wise choice was exercised in the selection of remedies adapted to special indications. Blood-letting by scarification of the lids, or incisions into the chemosis, Atropia, nitrate of silver, or in sthenic cases a solution of Bromide of Ammonium, 40 or 60 grains to the ounce of pure Glycerin, applied to all parts of the conjunctiva by a camel's hair brush, were resorted to when occasion demanded. In addition to these means, Hancock's operation of division of the ciliary ring was performed whenever the cornea became anæsthetized, and the pupil could not be influenced by atropine, though *no* adhesions existed.

Any means by which greater safety can be insured in the treatment of such a rapid and disorganizing disease as purulent ophthalmia, is worthy of our most serious consideration. From the representation made, it would be well to give pressure a trial, and if the result gives as favorable a return as in the cases of the author, purulent opthalmia will lose most of its terrors.

C.

MISCELLANY.

TREATMENT OF EPIDEMIC CHOLERA.

We publish by request the following preamble and resolutions of the New York Academy of Medicine, regarding the treatment of Cholera:

WHEREAS, The New York Academy of Medicine has endeavored to promote, among its own members and throughout the medical profession, a spirit of exact and practical inquiry into the preventive and remedial treatment of epidemic cholera; therefore, be it

Resolved, That this Academy hereby expresses its confidence in the utility of general and specific hygienic measures, as the best means of protection against the pestilential prevalence of cholera in any locality where it makes its appearance; and that the most thorough scavenging, cleansing, and disinfection are absolutely necessary means of averting this pestilence in the cities and populous towns of our country at the present time.

Resolved, That in the judgment of the Academy, the medical profession throughout this country should, for all practical purposes, act and advise in accordance with the hypothesis (or the fact) that the cholera diarrhœa and "rice-water discharges" of cholera patients, are capable, in connection with well-known localizing conditions, of propagating the cholera poison; and that rigidly enforced precautions should be taken in every case of cholera to permanently disinfect or destroy those ejected fluids by means of active chemical agents; also, that with the same object in view, the strictest cleanliness of person and premises should be enforced upon all who have charge of the sick; also, that all privies, water-closets, and cess-pools, should be kept thoroughly under the control of disinfectants.

Resolved, That we regard the nature and causes of cholera infection, so far as the sick or their discharges can propagate it, as being so susceptible of control that there should be no fear or hesitancy in the personal care of the sick and all that pertains to them.

Resolved, That immediate and thorough cleansing, and disinfection of all persons, clothing, and things that have been exposed to the discharges or persons of the sick with cholera, constitutes the chief end and object of any rational quarantine or external sanitary police regulation against cholera.

Resolved, That, for the purposes here mentioned, an external sanitary police is desirable in all great maritime and river towns, but that such sanitary regulations need not seriously embarrass commercial intercourse and the interests of trade.

Resolved, That the main source of protection against epidemic cholera at the present time is to be found in the vigilant and effective operation of sanitary measures, municipal, domestic, and personal.

Resolved, That the New York Academy of Medicine cordially invites the physicians of every city and village throughout our country to urge the immediate adoption of adequate measures of sanitary protection against the introduction and ravages of cholera, and that to this end we pledge our

The above resolutions were unanimously adopted by the Academy.

MISSOURI GENERAL HOSPITAL.

Hospitals, like all institutions, of philanthropy or mere public utility, are indebted for their existence to a conscious necessity in the individual and public mind. This necessity for many years has been sadly experienced by many in our city, denied the comforts of home and the ministrations of kindred,

whom sickness or accident may have overtaken. Physicians have observed the want of these powerful auxiliaries, both in treating their patients and expediting their convalesence. In response to this necessity, and for the purpose of providing these aids, the present measures were instituted for the establishment of a General Hospital in St. Louis.

In institutions of charity this city is not a whit behind her sister and elder cities at the East; and her eleemosynary institutions assume to provide amply for her sick and suffering *poor:* neither of these, therefore, meet the demand designed to be supplied by the hospital now initiated. To this, the hospital of the "Sisters of Charity," in its scope and extent, more nearly than any other corresponds.

The Missouri General Hospital was but lately organized by the election of a full board of officers and corporators, and will, no doubt, be carried on to a complete success, as its praise-worthy object demands. It has received the lively attention of the Mayor of the city, and many prominent physicians and surgeons, and worthily claims the cordial support of every good citizen.

It is designed to be founded and conducted upon such a basis as shall render it self-sustaining; adopting such a system of endowment of beds, and adjusting the rates of charges incident to general admission, either in wards or in private apartments, to such a scale as shall simply compensate for the expenses incurred: this fundamental principle being deemed indispensable to secure within itself the principle of its own prosperity.

The Hospital has now progressed to the stage of being duly incorporated, and embraces in its corporation some of the most public spirited and philanthropic men of our city. These elements give a highly promising aspect to the undertaking, and are an earnest of its complete establishment, at the earliest practicable period. Much time and labor, however, must be expended before the first visible blow can be struck for the erection of the edifice. This preliminary work, like the foundations of every permanent structure, is chiefly out of sight. But when the time for public action shall arrive, we are convinced from the na-

ture of the effort, the history of its inception, the character of the men engaged, the acknowledged spirit of philanthropy and liberality of our moneyed men, that an institution will be achieved commensurate with the demand, honorable to its founders and donors, and worthy the name and reputation of our great city, which augurs fair to attain, within the next decade of years, to the *third* rank in population among the American cities.

We have been pleased to see and examine the following instruments, invented by our fellow-laborer, Dr. George Syng Bryant, viz:

A self-retaining speculum, with Sims' duck-bill blade attached, and so constructed as to adjust itself to any sized woman.

A canulated needle, for introducing wire sutures in vesico-vaginal, or recto-vaginal fistules, in any part of the vaginal walls.

A needle for making the deep or central stitch in perineal lacerations.

A director for introducing the pin, in pin and figure-of-eight sutures.

A forceps, or double tenaculum, for bringing down and steadying the uterus in operations on its os and cervix.

All the above instruments were made by Leslie & Co., from diagrams.

A chloroform inhaler, so modified that chloroform can be administered for any length of time with perfect safety, to women in labor, or in any other condition requiring its use.

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All of these may be seen at the depot of A. M. Leslie & Co., Chesnut street, near Fifth street, St. Louis.

ST. LOUIS MEDICAL COLLEGE.

The Faculty of the St. Louis Medical College take pleasure in announcing that the last class exhibited a gratifying increase. Nearly two hundred students were in attendance during the session.

The ensuing COURSE OF LECTURES will commence on Monday, the first of October next, and continue until the following March; as also Clinical Lectures at the hospitals, during September.

These preliminary lectures are free of charge to all students, and those who can, will do well to attend them.

The great facilities afforded by St. Louis for Clinical Instruction, Medical and Surgical, are annually increasing.

Prof. Pope will also have regular Clinics on Surgery, during the entire session.

Graduates of other respectable schools are admitted to the Lectures on the payment of the Matriculation fee. The fees for the whole course (paid in advance), $105; Matriculating Ticket, (paid but once), $5; that of the Demonstrator, $10. The Hospital Tickets, gratuitous; graduating fee, $20.

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PROF. CHAS. A. POPE, M. D.

Dean of the College, Office corner Seventh and Myrtle streets.

FACULTY.—M. L. Linton, M. D., Professor of the Principles and Practice of Medicine; A. Litton, M. D., Professor of Chemistry and Pharmacy; Chas. A. Pope, M. D., Professor of the Principles and Practice of Surgery, and Clinical Surgery; M. M. Pallen, M. D., Professor of Obstetrics and the Diseases of Women and Children; Charles W. Stevens, M. D., Professor of General, Descriptive, and Surgical Anatomy; John B. Johnson, M. D., Professor of Clinical Medicine and Pathological Anatomy; J. T. Hodgen, M. D., Professor of Physiology and Medical Jurisprudence, and Dean of the Faculty; Frank W. White, M. D., Professor of Materia Medica and Therapeutics; E. H. Gregory, M. D., Demonstrator of Anatomy; L. Dinkler, Curator.

MORTUARY STATISTICS.

BY SOLON STARK M. D., CLERK OF THE BOARD OF HEALTH, ST. LOUIS, MO.

Abstract of Deaths occurring in the City of St. Louis, for the nine weeks ending June 1st, 1866.

WEEK ENDING	SEX. AGES.	Under 5 Years.	5 to 10.	10 to 15.	15 to 20.	20 to 30.	30 to 40.	40 to 50.	50 to 60.	60 to 70.	70 to 80.	80 to 90.	90 to 100.	Total.
April 6th.....	Males.....	22	4	0	1	0	4	7	3	0	0	0	0	41
	Females....	17	2	1	0	4	4	6	0	0	0	0	0	34
" 13th.....	Males.....	15	2	0	1	4	3	3	2	2	0	0	0	32
	Females....	17	3	0	4	5	5	1	3	4	0	0	0	42
" 20th.....	Males.....	20	1	2	1	3	6	5	4	0	0	1	0	43
	Females....	12	2	0	1	3	9	5	1	2	0	1	0	36
" 27th.....	Males.....	16	2	1	3	6	5	6	2	0	0	0	0	41
	Females....	13	3	1	1	2	1	1	2	2	1	0	0	27
May 4th.....	Males.....	16	2	1	1	5	6	4	2	1	1	0	0	39
	Females....	11	1	2	1	1	5	1	0	0	2	0	0	24
" 11th.....	Males.....	12	2	1	2	9	2	3	3	0	0	1	0	35
	Females....	15	1	2	2	3	6	2	1	0	2	2	0	36
" 18th.....	Males.....	16	2	2	1	3	5	7	2	3	0	0	0	41
	Females....	9	3	1	1	9	2	2	1	0	0	3	0	31
" 25th.....	Males.....	15	2	0	1	9	7	2	5	2	1	0	0	44
	Females....	15	2	1	1	5	4	2	0	0	1	0	0	31
June 1st.....	Males.....	15	3	0	0	7	8	1	1	0	0	0	0	35
	Females....	12	3	0	2	3	8	3	0	2	0	0	0	33
Total.....	.....	268	40	15	24	81	90	61	32	18	8	8	0	645

In addition to the above, persons died, ages unknown..... 4
Still Born..... 48

Total..... 697

Statement Showing the Diseases resulting in Deaths, for the Nine Weeks ending June 2d, 1866.

Asphyxia, 2; Accidents, 8; Apoplexy, 9; Arachnitis, 2; Abscess, 3; Anasarca, 1; Angina, 1; Asthma, 1; Anæmia, 1; Atrophia, 1; Ascites, 6; Burns, 2; Bronchitis, 10; Congestion of Lungs, 8; Croup, 9; Congestion of Brain, 26; Congestion, 2; Cholera Infantum, 2; Concussion of Brain, 4; Congestive Chill, 1; Colic, 1; Congestion of Liver, 11; Convulsions, 55; Cancer, 7; Cramps, 7; Cerebritis, 6; Cerebral Congestion, 1; Coma, 1; Dysentery, 8; Drowned, 4; Diarrhœa, 28; Died in Child Birth, 1; Diphtheria, 11; Erysipelas, 13; Endocarditis, 4; Enteritis, 9; Epilepsy, 3; Emphysema, 1; Encephalitis, 4; Exhaustion, 1; Empyema, 1; Fever, 8; Fever, Typhoid, 19; Fever, Intermittent, 1; Fever, Puerperal, 10; Fever, Lung, 1; Fever, Congestive, 3; Fever, Remittent, 2; Fever, Bilious, 1; Fever, Gastric, 1; Fracture, 1; Frost Bite, 1; Gun Shot Wound, 1; Gangrene, 4; General Debility, 26; Hydrothorax, 2; Hepatitis, 11; Hydrophalus, 9; Heart Disease, 7; Hemorrhage, 4; Hypertrophy of Liver, 1; Hydrops, 8; Hernia, 1; Hemoptysis, 2; Inflammation of Lungs, 7; Icterus, 1; Intussceptio, 1; Imperforate Rectum, 1; Intemperance, 1; Inflammation of Trachea, 1; Inflammation of Bowels, 8; Laryngitis, 3; Metroperitonitis, 1; Morbus Brightii, 1; Moribund, 3; Meningitis, 14; Marasmus, 16; Mania a Potu, 3; Not Stated, 13; Nephritis, 1; Necrosis, 1; Otitis, 1; Old Age, 5; Phthisis, 94; Pertussis, 6; Poison, 1; Paralysis, 5; Peritonitis, 4; Pneumonia, 54; Scirrhus of Liver, 1; Scald, 1; Scrofula, 1; Small Pox, 5; Suicide, 8; Syphilis, 2; Scarlatina, 5; Tonsilitis, 1; Tetanus, 5; Tuberculosis, 11; Typhus, 5; Trismus, 10; Tumor of Uterus, 1; Ulceration of Bowels, 1; Variola, 6; White Swelling, 2.

A WORD TO OUR PATRONS.

The present number of the Journal has been delayed in its issue at the proper time, in consequence of the late fire, which destroyed the greater part of the type that had been set, requiring us to re-do the work. We trust our friends will appreciate our difficulty, and find in the present number enough practical matter to justify all the delay.

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All communications should be addressed to Frank W. White, M. D., box 3060.

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SY/E---CHOICE OF GLASSES.

(No. 1.) benefit of his Pupils.

CLASSIFICATION OF EYES ACCORDING TO THEIR POWER OF REFRACTION.

read No. 1 or 2 of Giraud-Teulon
Very soon there is indistinctness

stance.

the external angles of the eyes.
bout the forehead and temples

n of the conjunctiva and retina.

lity of distinguishing small ob-
prins.
must be held at from 12 to 14 in.
good.
ying, of a strong light. Presby-
face the light between their eyes
marres, Father.)
cted.
in emmetropia toward the age of
; it is more rare in myopia and

nce to presbyopia; it is only re-
rapid progress, and the age of

nct. At a distance it is normal,
myopic or hypermetropic eye.
ng of the pupillary motions.

CAUSES.

a. Difference of focus.
b. Muscular asthenopia.
c. Opacities of cornea.
d. Chloro-anœ

TREATMENT.

a. Use convex glasses, Nos. 80, 72, 48 or 42.
b. Correct the asthenopia and differe of foci.
c. Frequent rest of the eyes during tl use.
d. Good regimen and tonics.

TREATMENT.

Necessity of selecting suitable glas at the very beginning.

Moderate Presbyopia.	Marked Presbyopia.
If the distance of distinct vision is not greater than 12 inches, Nos. 36, 30, 24, 20 or 18 biconvex.	The distance distinct vision greater than inches, Nos. 14, 13, 12, 11 a 10 biconvex.

CAUSES.

a. Diphtheritic, syphilitic or cerebral affections. (Follin.)
b. Instillation atropine.
c. Traumatic cau

TREATMENT.

Remove the cause and accustom the ey to study with No. 10 biconvex, a subsequently with feebler glasses.

sed by the contraction of the accommodating muscle, spontaneous or arti
or the calabar bean. Myopia results (Graefe). The eyes are much fatigu
parations of belladonna, either by frictions or in collyria.

hypermetropia is ordinarily less marked than the following.
ts degree, take the number of biconvex glass which will enable the patie
tly the characters C C of Giraud-Teulon at the distance of 20 steps. Th
ndicate the degree of hypermetropia. The number twice as strong is usual
needed for reading or minute work.
ance, there is generally no need of glasses in this variety.

AL. This is very rare, and often accompanied by a marked degree astigmatism. This anomaly is corrected by the simultaneou use of strong cylindrical and spherical glasses, Nos. 6, 5, 3, 2 1-2 and 2 convex.

s oper- ot or a lens. In the great majority of cases there is need of No. 2 1-2 biconve glasses for reading, and of No. 5 to see at a distance. F myopic persons, often a weaker number will suffice.

opia. Vision at a distance is accomplished by exaggerated contractions this muscle is paralyzed, or the crystalline lens grows dense by age, th cases have often been considered as an acquired hypermetropia.

THE SAINT LOUIS

Medical and Surgical Journal.

SEPTEMBER AND OCTOBER, 1866.

Original Communications.

THE PRESENT STATE OF THE DOCTRINE OF FEVER.

BY G. BAUMGARTEN, M. D.

Liebermeister, in his recent articles on fever, quotes the dictum of Van Helmont (1707): so many authors had already written "on fever," that one should almost feel ashamed of such a title.

Nevertheless, I am of opinion that a review of the recent labors in this field, of the facts with which we have become acquainted within the last decennium, and an examination into the present state of the theory of fever, are very pertinent and profitable studies. I have been led to this conviction chiefly by the casual perusal of our latest text-book on the principles and practice of medicine,*—a book bearing date of 1866, by an author considered by most as the representative American writer on the subject in our day. In the general part of the work, Dr. Flint avoids all mention of fever, but notices it in the special part in introducing the subject of "Fevers," thus:†

* FLINT. A treatise on the Principles and Practice of Medicine. Philadelphia, 1866.

† The points to which especial attention is directed are *italicised*. The quoted sentences have principal reference to "essential fevers." I have been unable to discover in the work any attempt at an explanation of symptomatic fever.

" . . . . Concerning the essential nature of these special causes [of fever], their generation, the manner of their entrance into the system, and their primary action within the body, our present knowledge is very limited." (p. 695.) . . . . "Of the nature of the pathological condition which constitutes an essential fever, *we have no positive knowledge.* . . . . The answer to this question [which is primarily affected, the nervous system or the blood?] which would be given by most pathologists at the present time is, the blood is the portion of the body first affected, and the essential pathological condition in fever is here seated. *The correctness of this doctrine is not demonstrable. It has not been proven by direct observation* that the special causes, giving rise to the different species of fever, act primarily upon the blood. But the doctrine is most consistent with our existing knowledge." (p. 696.)

Flint then proceeds to give eight (probable) reasons for considering fever primarily a blood disease, for the details of which I must refer the reader to the work itself. In brief, the author gives us not one decided expression of opinion, seeming to follow a non-committal course, and has no more positive support for the doctrine, than that it is "most consistent with our existing knowledge."

In refreshing contrast with this wordy negative exposition, is the concise definition in the contemporary work by Tanner*:

"*The cause of fever is the contamination of the blood by some morbific agent.* When this change—the nature of which is unknown—has proceeded to a certain extent, the researches of Dr. Parkes teach us that the nervous system, or rather that part especially connected with nutrition and organic contractility, begins to suffer alterations in composition. The muscles, and probably some of the organs, deprived more or less of nervous influence, begin to disintegrate, this disintegration producing undue heat; the condition of the vagus and vasi-motor nerves induces increased action of the heart and dilatation of the vessels; the contaminated blood is still further deteriorated by receiving the rapidly-disintegrating tissues by the continued action of the morbific agent, as well as by the functions of the lungs, liver, spleen, etc., being impeded; while, as no food is taken, the various alkaline and neutral salts no longer pass into the system."

Tanner, however, does not enter into any farther explanation of the process and phenomena of fever, nor give any substantiation of his definition and theory.

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* The Practice of Medicine. From the fifth London edition. Philadelphia, 1866. (p. 149.)

It has always been a matter of surprise to me to find the theory of fever untouched, almost avoided, in such works on general pathology as those of Simon* and Paget, the pages of which are in every physician's hands, the praise of which on every tongue; yet, seeing what strides of progress have been made within the last ten years or more in our actual *knowledge* of the nature of fever and the mutual bearings of its phenomena, it has surprised me more to see the pages of a work of to-day *touch* upon the subject, indeed, and yet so barren of true information.

Truly, as Dr. Flint says, our knowledge is limited; many phenomena of fever remain unexplained; but still that knowledge amounts to much more than the author quoted allows to appear. And for the very reason that we know so little, we should grasp and hold with tenacity all the *positive* knowledge we have acquired thus far. And let it not be said that a correct and definite theory of fever, however imperfect, is of no practical use at the bedside! To the educated practitioner, a theory will never be a source of confusion,—it *may* prove an invaluable guide.

Very frequently, in the study of fever, the results of investigation are impaired by regarding it from too isolated a point of view, having regard chiefly or only to the so-called essential fevers. Indeed, this is choosing the most complex object. Fever is the same, whether symptomatic of an acute inflammation, or of a wound, or of a "zymotic disease." And surely the simplest and least embarrassing object of study—the least complicated and obscured form of the process—is *traumatic* (simple inflammatory) *fever*. Some of the later investigators acted upon this idea, choosing traumatic fever for their observations, and applying to it the exact method of *experiment*.

It is unnecessary here to revert to the manifold researches of Zimmermann, Traube, v. Baerensprung, Parkes, Handfield Jones, Claude Bernard, Wunderlich; to the very recent, exact, and pro-

*Simon's Monograph on Inflammation in Holmes' System of Surgery, Vol. I. (London, 1880), treats fully on the subject of Inflammatory Fever; his views will be mentioned further on.

gressive labors of Billroth,* O. Weber,† Liebermeister,‡ and others, I would direct the reader's attention.

Weber defines (traumatic) fever as "a general increase of organic change, combined with elevation of temperature, which is caused by a contamination of the blood with the products of disintegration of the tissues, and leads to rapid diminution of the weight of the body." Serving myself of this definition as a convenient basis, I propose to examine the different parts of the proposition here presented in the order of their sequence as causes and effects.

1. *The cause of fever is a contamination of the blood.*

That a poisoning of the blood is the direct cause of fever in the case of small-pox, scarlatina, in typhus, in malarial fevers, has long been conceded; not so in the case of traumatic fever, or of the fever accompanying simple non-infectious inflammations. The *direct* proof of a similar causation in cases of mechanical injury has lately been furnished by Billroth, who was the first (Virchow's experiments relating to septicæmia excepted) to apply to this question the test of experiment on a large scale, and by O. Weber. Both have almost contemporaneously, but independently, established the fact that the introduction into the blood (of a healthy animal), not alone of putrescent serum of blood or pus, but also of the fresh serum of pus,§ of the fluids expressed from exudations and inflamed tissues,‖ of dried pus,¶ and of infusions of decomposing vegetable matter,** causes fever. Moreover, the injection of perfectly fresh and still warm pus into the subcutaneous connective tissue, or into the pleural cavity, was *rapidly* followed by a considerable febrile increase of temperature. And finally, the

* Beobachtungsstudien über Wundfieber und accidentelle Wundkrankheiten. 1st Article: *Archiv f. Klin. Chirurgie*, Vol. II. (1861.) 2d Article: *ibid.* Vol. VI. (1864.)

† Pitha und Billroth, *Hdb. d. allgem. u. spec. Chirurgie.* Vol. I. Erlangen, 1865; and *Deutsche Klinik*, 1864, 1865.

‡ Klin. Untersuchungen über das Fieber, etc. 1st Article: *Prag. Vierteljahrschrift*, 1865. Vol. I. 2d Article: *ibid.* Vol. III.—Ueber die Wirkungen der febrilen Temperatursteigerung. *Deutsches Archiv f. klin. Med.* Vol. I., 1865.

§ Weber. *D. Klin.* 1864. No. 51. 1865. No. 3. Billroth, 2d article. *l. c.* pp. 392, 396.

‖ Weber, *ibid.* 1865, No. 5.

¶ Billroth. 2d article. p. 475, *et seq.*

** *Ibid.* p. 482.

injection of fresh defibrinated blood taken from an animal affected with a traumatic or other fever, into the blood of a healthy individual of the same species, likewise invariably produced fever, with an elevation of temperature sometimes exceeding that of the animal from which the blood was taken.* The elevation of temperature thus produced takes place within a few hours, and sometimes even attains its maximum within that time. Controlling experiments (with different fluids, with healthy blood, and otherwise) demonstrated that the fever thus artificially induced is entirely independent of the local injury inflicted by the injection, which does not lead to inflammation until several days later; that it is not caused by a simple transfusion of blood, nor by the increase of volume of the blood or its dilution, nor yet by embolic obliteration of vessels. Repeated injections of pus or serum puris, caused a continual remittent fever, making an exacerbation after each injection. These experiments are very extensive, and led to identical results at the hands of two experimenters, working independently of each other, whose scientific name leaves not the slightest doubt as to their trustworthiness.

We have before us, then, distinct evidence that some material contained in the serum of recent pus, introduced into the blood either directly or by absorption from subcutaneous tissue or the pleural cavity, excites fever; that the fluids expressed from inflamed parts, when introduced into the blood, cause fever; and finally, that blood thus infected (taken from any feverish animal), transferred to the blood of another individual, likewise produces fever. The nature of this material, whether molecular,† fluid, or gaseous, remains in doubt; its action is like that of ferments, since it causes the blood of an animal into which it is introduced indirectly, to acquire properties like its own: to become pyrogenetic, even in very small quantities.‡

---

* Weber. *D. Klin.* 1865. No. 4.

† Billroth thinks it is molecular, and gives very plausible reasons for his view. 2d article, p. 469. Cf. also the investigations of Panum. *Virch. Arch.* xxv. p. 441.

‡ Weber. *D. Klin.* 1865. No. 7. Résumé.

The logical conclusions are obvious. If the serum of pus, or the fluid exudations in an inflamed part, are absorbed *into the blood of the patient himself*, fever must be the result; (and that such is the case becomes obvious from the experiments of Weber,* which showed that the blood of a dog in whom fever is excited, e. g., by the fracture of a leg, acquires the property of exciting fever in another dog). Billroth makes it very probable that the absorption takes place by means of lymphatic vessels.†

We can not escape the inference, then, (*a.*) that *fever is caused by a contamination of the blood, and* (*b.*) *that the symptomatic fever accompanying simple inflammations and mechanical injury is the result of self-infection*, just as in the case of pyæmia, septicæmia, etc., — an infection as indubitable as that of small-pox.

In 1660, John Simon already indicated this doctrine (although without the foregoing experimental verification which elevates it to the level of a fact) when attempting to show "how it is that feverishness superadds itself to inflammation." The lucid and direct language of the great pathologist will justify me in repeating the paragraph:

"Almost without hesitation we may assume that the process spreads by common material contagion; that the returning fluids of the inflamed part, —its venous blood and its lymph, are the agents of constitutional infection. For if a part be developing preternatural heat and undergoing preternatural change, its venous and lymphatic out-flow must of necessity represent those excesses of heat and change; and this out-flow with all its belongings, material and dynamical, shedding itself without interruption into the blood, must straightway be diffused throughout the body. Not inactively, we may presume; for the blood receiving such increments of heat as the inflamed part thus contributes to its temperature, and receiving also that unwonted afflux of chemically-changing material which comes from the disorganizing textures, is doubtless itself stimulated to greater activity of change. And as it circulates throughout the body, so, to every texture which it supplies, there is brought more than common incentive to change; till, in proportion as the fever is great, every texture, according to its chemical mobility, gets to participate in the excitement, reflects as it were in a lesser scale the disorder of the inflamed part, and contributes a share to that over-production of waste-products and of heat which is characteristic of inflammatory fever."—[*Holmes' System of Surgery, Vol. I., p.* 52.

* *D. Klin.* 1865. No. 5. p. 43.
† 2d article. p. 442, *et seq.*

2. *Fever consists in a general pathological increase of organic change.*

It is generally assumed that the source of the increased heat in fever is an accelerated process of decay,—increased combustion. The strict proof of it rests upon the demonstration that fever-heat is really due to an *over-production* of heat,—a proposition which has been so generally adopted as self-understood, that it was almost a surprise to see it called into question.* Liebermeister shows by actual observation, that during fever, more heat is often superadded to the previous temperature of the body in a given space of time, than is normally produced in that body. (For instance, in a paroxysm of intermittent fever, the patient weighing 57.5 Kilogrammes, suffered a rise of temperature in 30 minutes of 2.31° C.,—equal to 110.2 cal.,† the capacity for heat of the human body being 0.83. Under ordinary circumstances a man of the same weight will produce, in 30 minutes, only about 45 calories; hence the production of heat by the patient amounted to about two and a half times that over and above the normal production).

The acceleration of organic change in fever is, moreover, directly proven by the increased excretion of waste-products, and by the loss of weight. Accurate investigations have established that more urea is excreted during fever,—at first *absolutely* more than by a healthy person, and always *relatively* more than would have been excreted by the same person under the same circumstances without the intervention of fever. The production of uric acid is also increased, as well as of sulphates and phosphates. Regarding the influence of fever upon the weight of the body, Weber's experiments on dogs demonstrated that the loss of weight in fever far exceeds the loss by inanition alone,‡ and that

* A single voice, though one which carries much weight with it, has been raised against it. TRAUBE (*Allg. Med. Centralzeitung*, 1863, Nos. 52, 54, 102) considers the rise of temperature in fever due, not to an increased generation of heat, but to a diminution of the loss by radiation and evaporation, in consequence of the tetanic contraction of the smaller arteries.

† The caloric unity is equal to the amount of heat necessary to elevate one kilogramme of water one degree (Centigrade).

‡ *D. Klin.* 1865. No. 7.

even when food is ingested a very considerable loss can take place. Liebermeister's extended clinical observations confirm this proposition. We are safe, then, in designating increased organic change as the essence of fever.

The link between the contamination of the blood with the pyrogenic material and the preternatural change of matter, is still wanting; the chemical process by which the intoxicated blood initiates the more rapid oxydation is unknown.* The poison acts upon the blood and tissues after the manner chemists call catalytic, and the idea of comparing that process to the action of ferments, possesses much probability, but it must remain hypothesis for the present. Nor do we know whether or not the nervous system intervenes to accomplish it; but to assume such an intervention is not at all necessary to the explanation of the facts, and would not in the least alter the laws which have been elaborated so far. To decide the question whether the destruction of tissue is increased through the agency of nerves acted upon by the poison, or takes place directly in the manner of ordinary chemical processes, the facts now at our command do not afford sufficient basis.

3. *Fever-heat and its consequences.*

We have arrived at the pathognomonic symptom of fever, the *calor præter naturam*, recognized as such by the ancients. The introduction of thermometric measurement into clinical observations, and, later, of reliable accuracy into the thermometric methods themselves, has furnished us with trustworthy data on the temperature of fever. Careful investigation has established

* Wachsmuth (*Arch. d. Heilkunde*, 1865. 3. Heft) suggests the following theory: The immediate effect of the fever cause is an alteration in the diffusion between the blood-cells and the blood-plasma, in such wise that the induction and transmission of ozone is increased, and, by consequence, organic disintegration is accelerated; i. e., fever is produced. W. adduces the increased excretion of pigments in the urine of fever patients as a proof that the exchange of matter between the cells and the plasma of the blood does really take place in fever,—that actually greater quantities of hæmatine pass from the blood corpuscles into the plasma. The theory is based on the discovery of Schœnbein and Alex. Schmidt, that the blood-cells not alone carry and transmit ozone, but also induce it (change common oxygen to ozone); and that this induction and transmission of ozone is accomplished by the hæmatine alone.

36.2° C. (or 97.2° F.) to be the minimum, and 37.9° C. (100. 2° F.) the maximum of normal temperature, as measured in the axilla. The diurnal variations in the same individual never exceed 1.3° C. (2.3° F.).* *Any temperature above* 100° *F. may be considered as fever-heat.* The highest point to which the thermometer rises in disease is 42.5° C. (108.6° F.), a point, however, which predicts with certainty a fatal termination.† A temperature over 42° C. (107.6° F.) will cause death necessarily, almost without exception; and a temperature above 40.5° C. (105° F.), if it continues for any length of time, is a very unfavorable prognostic sign. (Wunderlich.)

It has been already stated that the great general source of fever-heat consists in the increased oxydation of the tissues; but there is another source which, though far less important, merits attention. Although Billroth asserts, on the basis of a number of experiments, that the inflamed part in a local inflammatory disease scarcely ever shows a measurable elevation of temperature above a corresponding healthy part, yet many other observers before and after him have arrived at a different result. The very accurate experiments of John Simon,‡ who compared the temperature of an inflamed part with that of the analogous healthy part by means of a thermo-electric battery and astatic galvanometer, have finally and conclusively demonstrated an increased development of heat in local inflammations; they were repeated, in a modified form, by Weber,§ with the same result. In one of the latter's experiments, the difference between the temperatures of a fractured leg and its fellow was found to be 2.5° C. (4.5° F.). The observations of Schroeder,‖ also, made on puerperal women,

* WEBER, P. and B. *Chirurgie*, I., 613.

† In the moment of death from tetanus, WUNDERLICH found the temperature in one case over 112°, and rising after death to nearly 114°, the highest ever observed. During the last moments of life, especially in diseases of the nervous centres, the temperature often rises suddenly, and sometimes continues to rise for a short time after death. (ERB, Ueber die Agoniesteigerung der Kœrperwærme bei Krankheiten des Central-nervensystems. *D. Arch. f. klin. Med.* I., 175.)

‡ *Holmes' System of Surgery*, I., p. 42. Note.

§ P. and B. *Chir.* I. p. 384-6.

‖ *Virchow's Archiv f. Path. Anat.* XXXV. p. 253, *et seq.*

and relating to inflammatory states of the uterus and vagina, gave a like result, and led their author to the conclusion, that although higher grades of fever are produced by oxydation, involving all the tissues of the body, minor elevations of the general temperature may be caused by a local production of heat alone. The inflamed part, therefore, is warmer than in the normal state. Now, *a priori* reasoning must convince us that a portion of this surplus heat is communicated to the general circulation and a practical argument in support of this supposition is the fact, that in extensive and violent inflammations the fever reaches a higher point, *cæteris paribus*, than otherwise.

The preternatural heat of fever, however, must no longer be considered as a symptom alone; it will be shown in subsequent pages that it is also a source of danger to the patient,—that it is in itself sufficient cause for numerous anatomical and functional lesions, and in many cases of febrile diseases the cause of death.

The *increased frequency of the pulse* is one of the results of fever-heat, and the most constant one. This proposition, as far as is known to me, has been first prominently stated by Liebermeister,* who proved by new and exact observations, that the increase of the frequency of the pulse generally corresponds in degree to the increase of heat;† not alone in fevers, but also in artificial elevations of temperature. The connection between the two phenomena herein shown, may be of three kinds: the frequent pulse may cause the heat, or the heat may cause the frequent pulse, or both may be the effect of a common cause. The later neuro-pathological theories of fever assume a connection in the first sense. There are positive reasons, however, for asserting that the accelerated pulse is a consequence of the increased heat. The following facts especially bear upon the question: (1) The pulse may be accelerated by *artificial* elevation of temperature, in the hot bath, the heated room, etc.; (2) the rise of tempera-

* *D. Arch f. klin. Med.* I., p. 461, *et seq.*

† Compare also UHLE and WAGNER, Hdb. d. allgem. Pathologie. 3d edition. Leipzig, 1865. p. 537.

ture in fever can generally be shown to *precede* the acceleration of the heart's action; and, (3) all those influences which diminish the temperature of the body,—cold, inanition,—are known to retard the pulse.

It is not to be understood that fever heat is the only cause of the frequent pulse; many other circumstances (prominent among which is the function of the nervous system) exert a large, sometimes paramount, influence in this direction. Nevertheless, the parallelism of the two phenomena is usually so accurate (Liebermeister, l. c., p. 466, based upon 4,205 measurements, states the acceleration to amount to 8 beats per minute to 1° C.=4½ beats to 1° F.) that, in a given case, a striking want of correspondence between them may be a proof to the practitioner that still another pathological influence is at work.

In this connection it will be of interest to mention, that the greater frequency of the pulse is by no means indicative of an acceleration of the circulation,—a proposition the reverse of which has usually been considered an axiom. To prove that the rapidity of the circulation is proportional to the frequency of the pulse, it is necessary to demonstrate that the quantity of blood propelled by each contraction of the heart remains the same, while the pulse varies. Undoubtedly, this is often the case. But, on the other hand, there are numerous cases in which the rapidity of the circulation sinks *below* the normal standard, even when the pulse is very considerably accelerated. For instance, while in the normal state (the tension of the blood within the arteries being relatively great) the influence of gravity is of small effect, we find symptoms of gravitation: hypostatic phenomena, in those very cases (of "asthentic" fever) when the contractions of the heart are excessively frequent; the labor performed by the heart is lessened; the tension of the arteries is reduced; the influence of gravity preponderates. Another phenomenon illustrating this point is the ominous symptom of coldness of the extremities and other peripheric parts at the same time with considerable increase of temperature in the trunk. This excessive inequality in the temperature of different parts of the body is a sure index of an

extraordinary diminution of the heart's action; for the temperature of peripheric parts depends chiefly on the quantity of blood circulating through them; the more rapid the circulation, the more nearly does the warmth of peripheric parts approach to that of the trunk, and every reduction of the rapidity of circulation must consequently reduce the temperature of distant parts more nearly to that of the surrounding medium. Internal parts, on the contrary, more prominently the sources of their own heat, are protected against losses by radiation or evaporation.

It is this diminished rapidity of the circulation which impresses upon certain cases of fever their *asthenic* character; and Liebermeister* thus expresses the relation between increase of temperature and asthenia: *Every fever in which the temperature continues to be considerably elevated, after prolonged duration assumes an asthenic character.* (As a general rule, a temperature of 40° C. [104° F.] is sufficient to effect this.) Asthenia is not always due to increased heat alone, however; obstructions of the circulation, general debility, or debility of the cardiac muscle, the influence of some poisons, etc., can give a fever the asthenic stamp. Clinical experience affirms this position throughout. Those infectious diseases, e. g., in which the fever always takes the asthenic type, are those in which a high temperature is wont to prevail for a long time; whereas, those in which cases of moderate fever and of short duration occur (smallpox, measles, etc.), do not present the asthenic character in the cases mentioned, unless it be due to other causes. Again: all cases of symptomatic fever which are marked by extreme elevation of temperature and long duration, follow the asthenic type. The pulse in asthenic fever is usually much more accelerated than in sthenic fever, and the greatest frequency of the pulse ever observed coincides with the highest degree of asthenia.

Now, since the elevated temperature causes asthenia (retarded circulation), and the latter is accompanied with great frequency of the pulse, Liebermeister is led to this conclusion: *Every especially large and prolonged increase of heat effects a*

* *D. Arch.* I., p. 475.

paresis (incomplete paralysis) *of the heart, which is manifested in very great frequency, together with disproportionate feebleness of the contractions of the heart.* This paresis passing into complete paralysis, causes the fatal termination of the case.

How the paresis and paralysis of the heart are effected, is a question perhaps not quite ready to be solved. One would be led, at first thought, to the supposition that the excessive heat impaired the function of the heart by its influence upon the cardiac nerves or their central origin. But the results of experimental inquiry do not confirm this view. The heat exerts its influence directly upon the heart. The extensive experiments of Panum show that the temperature determines the number of contractions of excised hearts (of frogs, rabbits, etc.), and that the excised heart loses its contractility and excitability the sooner, the higher the temperature. In man, the frequency of the pulse in cardiac affections, as well as in fevers of high grade, can be reduced by the energetic application of cold to the præcordial region. It is highly probable, then, that the parts acted upon by the heat are situated in the heart itself; they are either the nervous apparatus of the heart, or its muscular structure. We shall see below that in cases of high fever the latter suffers actual anatomical change, and is probably the organ mainly implicated.

The *symptoms on the part of the nervous system* can not be referred to their source with any similar precision; for the subject is devoid of absolute proof, because the functions of the nerve centres are not so susceptible of mathematical measurement as the heart's action; we have no thermometer, no sphygmograph, no chronometer, to apply to the action of the brain. *A priori*, there is no reason to believe that a morbific material introduced into the blood may not, besides altering the chemical activity of the blood, act upon the nerve centres also; but neither is there any valid argument showing that this *is* so in fevers. Nevertheless, it seems to be the almost universally adopted doctrine in our day, that the nervous symptoms of fevers,—especially the severe forms, delirium, stupor, coma,—are brought about by the direct

action of the poison upon the brain and medulla, or by some secondary toxæmia generated in the course of the fever. (Lebert.) Again: organic decay is accelerated not in the blood, or the muscles, etc., alone, but in the brain also; increased oxydation must lead to increased function, increased action in some direction, however perverted; therefore the organic change, aside from the heat it generates, may be conceived to suffice for causing at least those nervous symptoms usually denominated symptoms of excitement; and there is no evidence that this is *not* so. Liebermeister attributes (as Niemeyer and Traube have also done) the nervous disturbances of all diseases attended with fever to the direct effect of fever heat.* The mildest form of these symptoms (restlessness, incapacity for mental exertion, the feeling of soreness and aching throughout the body, and headache) can be produced artificially by elevating the temperature of the body, and no doubt can be entertained but that those symptoms are due to the heat. And in certain cases of disease it is easy to observe with the greatest accuracy the transition from these lighter forms of cerebral disturbance to the severer phenomena of delirium, sopor, coma, etc., so that it becomes probable that they also would be induced in a healthy person, if an artificial elevation of temperature were carried far enough. On the other hand, by an artificial depression of temperature we are able temporarily to interrupt the delirium of a fever patient. Typhus fever, the disease in which all the nervous phenomena of the febrile state are most constant, most distinct, and commonly attain great severity, is also the very disease in which we are wont to observe the highest temperatures for the

* *D. Arch.*, I., p. 543, *et seq.* The arguments adduced by him are, in my opinion, conclusive; but they apply equally well to the heat, *per se*, and to the chemical process producing it, the too rapid oxydation. I am not quite prepared to assert that the *delirium* of fever is always the direct result of the increased organic decay; and as it was my object in this paper only to relate the opinions prevailing among those now foremost in the study of fever, I will confine myself to indicating briefly that Liebermeister's arguments will prove as much in favor of causation by the organic change (which precedes and *produces* the heat) as for the origin by heat. For the rise of temperature with which the delirium coincides, is itself coincident with increased oxydation: the latter is necessary to the production of the heat; it is also necessarily followed by some increase (or perversion?) of function, and this is expressed as delirium.

longest period. The most valuable fact bearing upon this question is, however, that these functional disturbances are entirely independent of the nature of the disease which they attend; that they are common to the most widely different diseases, provided only that the temperature of the patient reaches a sufficient height.* All these reasons make it very plausible, even though they do not strictly prove, that *the functional disorders of the nervous centres in fever are owing mainly to the excessive heat.*

The two series of functional derangement just discussed,—the frequency of the pulse and the nervous symptoms,—are undoubtedly the foremost effects of fever-heat. Within the last years, however, Liebermeister has shown that the influence of febrile elevations of temperature is manifested, not in lesions of function alone, but that it has also *anatomical* results. In the course of studies unconnected with the subject of fever, he had found a number of cases† in which a secondary parenchymatous degeneration (molecular and fatty) of the liver was attended with like changes of the kidneys and heart, and sometimes of the spleen. Pursuing this subject farther, he observed these alterations in many more cases and in very different diseases; namely, in pyæmia, puerperal fever, typhoid, scarlatina; also in acute miliary tuberculosis, peritonitis, acute articular rheumatism, and even in a severe case of simple catarrhal fever. These degenerative processes, then, were not peculiar to any one disease, nor were they peculiar to infectious diseases alone. The common cause of the parenchymatous degenerations must therefore lie in circumstances common to all those diseases; and the only point the cases observed *all* had in common, was the high grade of the accompanying fever. Indeed, the majority of the cases noted had been marked by excessive elevation of temperature.‡

* I need scarcely remark that no superficial experience or specious theorizing has led to these statements, but that they are based on numerous and exact experimental and clinical observations, in which the degree of fever-heat is diligently noted by aid of the thermometer. Liebermeister has met with delirium in simple intermittents, in erysipelas (independent of cerebral complication), in angina, and even in a case of simple catarrhal fever.

† Beitraege zur patholog. Anatomie d. Leberkrankheiten. Tuebingen, 1864.

‡ *D. Arch.* I., p. 328, *et seq.*

If these cases, collected by but one observer, and therefore not numerous enough for statistical purposes, are very far from deciding so intricate and important a question, they are yet of the very highest value in connection with some other facts known to us. We have been taught by the researches of M. Schultze, W. Kuehne, and others, that elevation of temperature exerts a deleterious influence upon the life of the cell; that all vital phenomena of cells are annihilated by the application of a certain degree of heat (49° C., or 104° F., in the frog). And certain facts in medicine have been known of old, or recently discovered, which give essential support to the doctrine of Liebermeister. Louis, Stokes, and other observers, have called attention to the soft, flabby, pale condition of the heart,—*post-mortem*,—in typhus patients (especially subject to excessive fever-heat). E. Wagner has observed pronounced fatty degeneration of the heart in nine cases of typhoid, which all had succumbed to a *fever of high grade* (*not* perchance to any sudden fatal accident in cases of moderate fever). Buhl found parenchymatous alterations of several organs in typhus and in pyæmia, and attributed them *to the fever as such*. Zenker discovered extensive degeneration of the voluntary muscles to occur almost constantly in typhoid fever, but also in other severe febrile diseases, and expresses it as his opinion, that the above lesion is one of the phenomena of *fever*. These facts are certainly of great importance, and lend a high degree of probability to the proposition, that the secondary parenchymatous degenerations of the liver, kidneys, heart, spleen, and voluntary muscles, often occurring in severe febrile diseases, are in many cases the immediate consequences of the high temperature. (For Liebermeister is not ready to assert the latter as the only cause of the processes mentioned. They seem to me to be closely related to the degenerative processes occurring in the habitual drinker.) Although this theory is based only on the frequent coincidence of the degenerations with excessive heat, yet it is borne out by the knowledge that a certain high temperature in all higher animals inevitably leads to death, and that living cells will not endure a degree of heat above a certain definite

point, which lies within the range of febrile temperatures actually observed; and, moreover, it is a theory explaining the respective facts by the aid of fewer hypotheses than any other yet proposed.

There are still other local effects of fever, such as the invariable presence, in inflammatory fever (in animals the subjects of experiment—Weber*), of extensive hyperæmia of the mucous membrane of the intestinal canal, the frequent occurrence of vomiting, and of very copious watery diarrhœa, etc. Weber† also remarks that the injection into a healthy animal of blood infected with inflammatory products, not alone produces fever, but sometimes also local inflammations (of the lungs, serous membranes, etc.), independent of embolia, or the like. But these changes are, for the present, attributed rather to the direct effect of the blood-poison, than to the high temperature.

Our knowledge of other effects of fever heat is limited and somewhat uncertain. It is probable that the tendency to multiple hæmorrhages, which often accompanies the more violent forms of various fevers, is one of these; Liebermeister quotes Sydenham in support of this idea, who invariably ascribes the hæmorrhagic diathesis in small-pox, whenever he speaks of it in his works, to the excessive heat.

We conclude, then, from the labors of Liebermeister,—who has done for the interpretation of the nature and symptomatology of fever, what Weber and Billroth have accomplished for its pathogenesis,—that one of the greatest dangers of fever consists in the excessive heat produced by it. The limits of this article, already extended, I must fear, beyond the reader's patience, will not permit me to follow the instructive remarks of the learned observer on the subject of the "dangers" of fever; but there are some other considerations of the utmost practical worth, which it would be unwise to overlook in the present state of our science, and to which I will briefly revert. I consider it one of the greatest achievements in this branch of pathology, that we have

* P. and B., *Chirurgie*. I., p. 605.

† *Ibid*. p. 610.

become able, by means of the researches detailed above, to explain the nature of the "*typhoid state*," and the causes of the "*malignity*" of certain diseases.

The true character of "asthenia" occurring in fever can be referred, as we have seen, in most cases to a paresis of the heart, or feebleness of the heart's action due to the excessive febrile heat. The nervous symptoms, and the peculiar impairment of the mental functions, constituting the chief characteristics of the typhoid state, have also been shown to be based upon the great heat of high fevers. The parched tongue, the sordes upon the teeth, are also naturally referred to the influence of heat in a patient unconscious or comatose. The enormous thirst (the perception of which is blunted by virtue of the nervous depression, but which nevertheless continues to be present in the typhoid condition) is, of course, due to the same cause. It is reasonable to conclude, then, that *the typhoid character of a fever*, with all its essential peculiarities, *is the result of excessive elevation of temperature;* in other words, it indicates, and results from, the high grade of the fever; it differs from the milder manifestations of a moderate fever only in degree. The typhoid *state* is not a characteristic of certain diseases, of typhus, or typhoid, or "infectious diseases" alone, but may supervene upon any symptomatic or simple inflammatory fever, if the latter attains the necessary intensity and duration. In those diseases which are especially marked by the frequent occurrence of typhoid symptoms, and have thence derived their name, the typhoid state is not developed unless a continued high temperature is effected; and even after the patient has entered upon that state, an accidental or intentional (therapeutical) moderation of the heat for some time will lead to an abatement of the typhoid symptoms. Indeed, the natural lysis of a typhoid fever always follows, in point of time, after a reduction of the patient's temperature.

This exposition of the typhoid state will find an application in our view of typhus and typhoid fevers. If the former is no longer considered as essential to the nature of the particular disease in which it occurs, there is no reason left for placing

typhus and typhoid in the same group, or even for deeming them at all closely related. That which gives these diseases their similiarity consists exclusively in this: that both are infectious diseases, usually attended with fever of great intensity and long duration (typhoid phenomena); in all other respects they are thoroughly dissimilar.*

On the subject of the *malignity* of fevers, I will endeavor to repeat briefly the interesting ideas contained in Liebermeister's essay.† At all times, "malignity" has been predicated of certain severe forms of disease, or certain individual cases, with a peculiarly and *unexpectedly* severe train of symptoms; and there is no harm in the use of the term so long as it is remembered that it is not an explanation of the facts concerned, but only supplies the want of a brief mode of expression. There is no objection to making a distinction between a mild and a malignant epidemic of scarlatina, or to the designation of "pernicious intermittent," and similar terms. In our day, the view most current among those physicians who have formed a clear conception of the subject at all, seems to be that the malignant symptoms are the more or less direct effect of the specific virus upon which the (infectious) disease is based; the great similarity of these symptoms to those of many acute intoxications, is one of the facts supporting the doctrine; and the diversity of cases of the same epidemic, or of different epidemics of the same disease, in point of malignity, is satisfactorily explained by the presumed difference in the quality and quantity of the poison absorbed, and in the individual predispositions of the infected organisms. Still, there are serious objections to this hypothesis. In the first place, there is a surprising resemblance among the so-called malignant phenomena in all diseases, whereas the other symptoms, especially the local alterations, differ materially according to the specific virus. And, secondly, we observe that diseases not usually attributed to an infection (erysipelas, pneumonia, etc.) often take a similar course of malignity, with symptoms differing in no wise from

*Liebermeister. Zur Aetiologie d. Abdominaltyphus. *D. Klin.* 1866. No. 6.
†*D. Arch.* I., p. 302, *et seq.*

those in malignant cases of infectious diseases. Other hypotheses on this subject are based on the supposition that special poisons are secondarily developed in the system, causing the malign accidents; the final products of normal organic decay, ammonia, carbonate of ammonia, carburetted and sulphuretted hydrogen, acetone, etc., have been so accused. Liebermeister thinks that a more correct knowledge of the nature of fever will in a great measure abrogate the demand for such unfounded surmises. Cholæmia, uræmia, the retention of nitrogenous products of organic disintegration, and the chemical consequences of insufficient respiration, have been adduced to elucidate the origin of pernicious symptoms. The very diversity of ætiological moments, invented to explain different cases, while in all probability the source of the malignity is in most cases alike, indicates the insufficiency of these theories. But there is one moment common to the great majority of cases which takes an unexpectedly severe course: the excessive elevation of temperature. It would be narrow and one-sided to attribute *all* obscure cases of unusual severity to the same origin; but the great general cause of malignity in febrile diseases undoubtedly resides in the fever-heat carried beyond a certain point. After the above explanations on asthenia and the typhoid state, it will appear superfluous to dilate farther upon the subject here.

Taking a final review of the advance thus made in the pathology of fever, we find the process constituted of the following moments:

1. A morbific (pyrogenic) material is introduced into the blood, either from without, or generated in an inflamed part within the body itself.

2. The pyrogenic material, in a manner still unknown, increases the normal process of organic change, which results in an increased production of heat, and in a waste of the tissues greater than occurs in the healthy individual. This waste may become the cause of death by emaciation or exhaustion.

3. The increased production of heat leads to a general elevation of temperature, which again causes anatomical lesions

(usually microscopic) and disturbances of the circulation (greater frequency of the pulse, subparalysis of the heart, diminished rapidity of the circulation, asthenia, hypostatic phenomena, etc.) and of the functions of the nervous centres (lassitude, restlessness, headache, delirium, coma, etc.), constituting the chief danger of fever, which often results in death by paralysis of the heart and nervous centres.

THE CHOLERA IN PARIS IN 1865.

A Review of the Statistics of Deaths in the Hospitals during the months of October, November and December.

BY R. H. HAYNES, M. D., ST. LOUIS.

That which of late has most occupied the mind of the medical profession, both in Europe and America, and even of the world, is the presence, in an epidemic form, of that terrible scourge which once again threatens to take its course around the world; and, worse than the terrible and "grim-visaged war," that now "hath smooth'd his wrinkled front," to commit its usual ravages in killing the young and old, the beauteous and fair; for it makes no distinctions, and where it touches, pain and death are sure to follow.

It is not our hope or intention to give here any new idea upon the pathology and treatment of this disease, nor do we expect to

throw any light upon the subject; for too much has already been said and done, and too many ideas have been advanced upon the subject, and have been sustained by the eminent men of the profession, and they only lead to confusion; and the practitioner only becomes hesitating and undecided in presence of such an array of remedies which are all pronounced to be good and efficacious. All that we here propose to give is an account of the epidemic as it appeared in Paris, in 1865.

We are indebted to that excellent journal of medicine, "La Lancette Française," or "Gazette des Hôpitaux," for the statistics we give, and we feel confident that they can be relied upon, for they were collected with great care and attention.

The first appearance of cholera in Paris, during the year 1865, was in the month of June; but we can not say that it was epidemic cholera, for it did not assume that form until several months after, and all communications concerning the subject at that time rather indicated it as "cholerine," and not the Asiatic cholera. All of the statistics that we have been enabled to see, give for the month of June, in all of the hospitals of Paris, six cures and one death; for the month of July, twenty cures and one death; for the month of August, sixteen cures and one death. In the year 1864, the same disease had caused six deaths in the month of July, and three in August, that is, nine deaths in two months; while in the last year, only three deaths in three months; which fact tends to prove the non-epidemic character of the cholera in those three months, and it was not until the month of October that the cholera assumed a form resembling in its nature an epidemic. Unfortunately, we have not been able to obtain an account for the month of September, and the report from the numerous journals at that time could not be relied upon; for the disease had not assumed a form of sufficient gravity to excite any apprehensions as to its epidemic nature, and it is not until the 18th of October that we are able to commence our account. If the information that we have obtained be exact, the following is the account from that date:

Date.	No. Admissions.	No. Deaths.
Oct. 18	118	58
" 19	134	66
" 20	141	23
" 21	80	56
" 22	66	52
" 23	70	35
" 24	85	49
" 25	69	37
" 26	55	40
" 27	49	31
" 28	42	27
" 29	35	27
" 30	48	29
" 31	35	29
Nov. 1	54	35
" 2	56	22
" 3	53	25
" 4	38	22
" 5	23	12
" 6	34	19
" 7	37	18
" 8	44	21
" 9	39	18
" 10	25	15
" 11	28	7
" 12	22	12
" 13	28	15
" 14	24	14
" 15	33	12
" 16	29	13
" 17	25	10
" 18	33	17
" 19	30	14
" 20	34	10
" 21	29	9
" 22	17	17
" 23	13	14
" 24	15	6
" 25	15	6
" 26	12	2
" 27	20	7
" 28	21	9
" 29	17	7
Total	1875	1017

We are unable from the 29th of November to give the number of admissions into the hospitals, but have been so fortunate as to obtain the number of deaths in the hospitals from that date till the 25th of December. It will be seen that from the middle of November the disease has commenced to cease its ravages, and its victims are diminishing in numbers.

From the 30th of November, the the deaths in the hospitals were as follows:

Date.	No. deaths.	Date.	No. deaths.	Date.	No. deaths.
Nov. 30	4	Dec. 9	3	Dec. 18	10
Dec. 1	7	" 10	8	" 19	2
" 2	5	" 11	6	" 20	8
" 3	8	" 12	6	" 21	0
" 4	4	" 13	8	" 22	0
" 5	9	" 14	3	" 23	1
" 6	1	" 15	5	" 24	1
" 7	9	" 16	7	" 25	4
" 8	4	" 17	6		
				Total	105

The number of deaths in December being 105, and those of October and November, 1,017, makes the total number of deaths in the hospitals, 1,122.

On the 25th of December our account ceases, for the disease at that date had lost all of its terrible proportions, and soon came to be regarded as one of the things that were.

In our above lists we have given only the number of deaths in the hospital, and below we give those that occurred in the city and hospitals. Of course there was a considerable number in the city of which no account could be obtained; but the report is as exact as circumstances permit, and shows very satisfactorily to what height the mortality attained.

Our statistics for the city commence from the 6th of October, and shows the mortality in the city, hospitals included, and is as folllows: On the 6th, 75 deaths; the 7th, 98; the 8th, 112; the 9th, 150; the 10th, 183; the 11th, 165; the 12th, 191; the 13th, 180; the 14th, 196; the 15th, 264; the 16th, 216; the 17th, 215; the 18th, 229; the 19th, 187; the 20th, 206. It is much to be regretted that we have not been able to find the mortality for the three following days, but on the 24th we can again commence our numeration; on the 24th, 99 deaths; the 25th, 111; the 26th, 97; the 27th, 82; the 28th, 76; the 29th, 73; the 30th, 77; the 31st, 65; Nov. 1st, 128; the 2d, 105; the 3d, 106; the 4th, 116; the 5th, 93; the 6th, 77; the 7th, 73; the 8th, 85; the 9th, 73; the 10th, 72; the 11th, 56; the 12th, 30; the 13th, 61; the 14th, 50; the 15th, 52; the 16th, 45; the 17th, 43; the 18th, 50; the 19th, 57; the 20th, 33;

the 21st, 30; the 22d, 39; the 23d, 35; the 24th, 29; the 25th, 16; the 26th, 14; the 27th, 21; the 28th, 22; the 29th, 14; the 30th, 14; December the 1st, 17; the 2d, 9; the 3d, 11; the 4th, 9; the 5th (?); the 6th, 4; the 7th (?); the 8th, 6; the 9th, 5; the 10th, 15; the 11th, 16; the 12th, 16; the 13th, 13; the 14th, 13; the 15th, 11; the 16th, 19; the 17th, 14; the 18th, 16; the 19th, 8; the 20th, 12; the 21st, 5; the 22d, 5; the 23d, 5; the 24th, 8; the 25th, 6. This makes the total number of deaths from the 6th of October to the 25th of December, 5,224.

Looking back to the hospital account, it will be seen that from the 18th of October to the 29th of November, there were 1,875 admissions, and 1,017 deaths, giving a mortality of 54.24 per cent.,—more than one half. It is not probable that the mortality in the city was so great, for a large number of patients who were brought to the hospitals were already in a state of collapse, and beyond the reach of aid, and their condition and mode of life were not so good as those of the more wealthy classes who could be cared for at home.

The largest number of deaths were on the 15th of October, being on that day 264, and from that date the disease gradually ceased its ravages, and as the weather became cooler and winter more advanced, the mortality became less and less, and with a few fitful starts, as of a flickering flame about to be extinguished, ceased altogether.

There is no subject that has occupied the profession more than what treatment should be employed in this disease. Time prevents us at present from giving a review of the many varieties of treatment that have been employed; but in the next number we promise to give you an account of the treatment employed, and results obtained by M. Lisle, "Chef de l'Asile des Aliénés de Marseille." The treatment pursued by Dr. Lisle will be seen under Translations. Let it suffice to say here that M. Lisle has employed, with the best success that we have seen, the sulphate of copper, which he gives from the commencement of the disease. His prescription is as follows:

Sulphate of copper, 5 grammes; distilled water, 100 grammes. And with this solution, he has made the following, viz: Solution of the Sulp. of Copper at $\frac{1}{2}$, 1 gr. 50 cent.; Laudanum of Sydenham, gtt. x.; sweetened water, 120 gr.

This potion is given to the patient from the commencement, and, in very severe cases, by a teaspoonful every fifteen minutes; in cases of less gravity, a half a teaspoonful every half hour, and in mild cases every hours. This is continued until the warmth of the skin and tongue returns, and till the pulse becomes more full. After this, the dose is given every three or five hours, and is discontinued when the state of the patient justifies the belief that the period of algidity is at an end.

M. Lysle states that out of twenty-six patients submitted to the treatment of sulphate of copper, only five died. This result is the most satisfactory of which we have heard.

OCCLUSION OF THE BOWEL.

Complete occlusion of the bowel, preventing the passage of stercoraceous matter during twenty-one days, followed by recovery.

BY LEON J. WILLIEN, M. D., EFFINGHAM, ILL.

Was called to see Mrs. D. S., aged 23 years, of lymphatic temperament, rather delicately constituted, who had been suddenly taken with lancinating pain in the left iliac fossa, accompanied with obstinate constipation lasting six days. She informed me that she had used laxatives and cathartics to remove the obstruction, but without effect.

Being consulted on the sixth day of her illness, I prescribed the Croton Tiglium to be followed by enemas, in order to obtain a discharge from the bowels, as the constipation seemed to be the only cause of her suffering; but with no result. The decubitus of the patient was dorsal; high fever; pulse weak and compressible—pulsations between ninety and one hundred per minute; great restlessness; respiration thoracic and short; face pale, indicating distress; eructations frequent; abdomen meteorized, but no hiccough.

One grain of extract of belladonna, in pill, was ordered to be given every three hours, and as soon as the above symptoms subsided, to be followed by a mild laxative.

On the seventh day found that she had vomited the above medicines, mixed with bile. Great agitation existed; the face was pale and bathed in perspiration; pulse small, compressible, and one hundred and twenty; urine deep red and scanty. She still suffered from flatulent eructations, with severe spasmodic pain in the above described region. In the evening the same symptoms presented themselves, with the addition of stercoraceous vomiting. The important point in this case was, to establish, as far as possible, a correct diagnosis in regard to the cause of the intestinal obstruction, and the portion of the bowel involved.

Careful exploration of the abdomen led me to discover a tumor of the size of a hen's egg, movable to a certain extent, yet very limited, elastic, distending more during the peristaltic movement of the bowels, causing at the same time great pain. The tumor was situated in the left side, between the crest of the ilium and umbilicus. After a close consideration of the various causes producing the train of symptoms, I concluded that the case was one of twisting of the bowels, and that, consequently, the prognosis was unfavorable.

As to the treatment to be followed, I proceeded—1st, to calm, if possible, the irritation of the stomach and bowels; 2d, counteract the inflammation and meteorism; 3d, endeavor by all means to obtain a passage through the bowels, naturally or artificially. The ext. belladonna was again prescribed, with the ad-

dition of sulphuric ether, in case of strong hiccough; refrigerent applications to the abdomen, narcotized to calm the excessive pain; and cold water, to be injected, per rectum, as high as possible.

On the tenth day she had obtained no relief from the above treatment. The symptoms were aggravated; pulse small—one hundred and thirty-six and compressible, great anxiety, slight delirium, strong dyspnœa, meteorism increasing. Ordered iced lemonade for drink, with a continuation of the above treatment.

From the eleventh to the thirteenth day the symptoms were less alarming. Vomiting of stercoraceous matter had ceased; bilious vomiting less frequent; meteorism *in statu quo*.

From the thirteenth to the nineteenth day, same state and same treatment.

At twentieth day the bilious vomiting had nearly subsided; pulse one hundred, yet weak; meteorism and pain less.

The idea of using taxis upon the tumor struck me, and I immediately proceeded to employ it, with the hope of obtaining, perhaps, some good result. Placing the patient on the right side, the thighs being flexed on the abdomen, I employed for about twenty minutes a gentle taxis upon the tumor, which was yet very painful. The same operation was repeated in the evening; treatment, ut supra.

Twenty-first day. While submitting the patient to the same mode of taxis, of about fifteen minutes duration, she was taken with a severe pain, during which the tumor melted away under my fingers, and at the same time I could perceive a strong peristaltic movement of the bowels. Demanding rest she was replaced in bed; but not twenty minutes had elapsed when the bowels were evacuated copiously and freely of dark colored liquid fæces. The pulse shortly after fell to seventy-five pulsations, became stronger, with complete apyrexia. The digestive organs returned to their physiological functions, and the patient was gradually restored to her ordinary state of health.

CASE OF FRACTURED PATELLA SUCCESSFULLY TREATED BY THE APPLICATION OF A RING.

BY W. A. GIBSON, M. D., ST. LOUIS.

JAMES FERREE, aged 35 years, had been the victim of rheumatism for five years, the last two of which had rendered him as helpless as an infant. I commenced treating him for the rheumatism on the first of January last, giving him iodide of potassium a drachm three times a day, with wine of colchicum and cod-liver oil. He improved rapidly under the treatment. The nodes on the extremities disappeared; the joints became flexible; and the ulcers which had formed on the feet and legs ceased to discharge, and speedily healed. This was a case of genuine idiopathic rheumatism, there being no venereal taint. On the 3d of May, he had so far recovered as to go about without his crutches, and as he attempted to get into a buggy, felt a sudden shock as if some one had hit him with a stone, on the knee, and it was with difficulty that I succeeded in prevailing on him that the fracture was caused simply by muscular action.

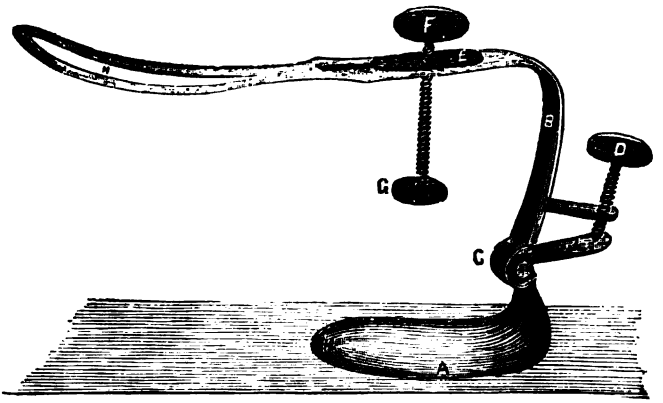
The patella of the left knee was fractured transversely through its middle, the fragments separating about one inch. I did not admire any of the modes of treatment for the fracture which I had ever seen recommended or used, and while hesitating as to what plan I would adopt, it occurred to me that a ring would answer the purpose. I therefore took a measurement of the sound patella, and had a ring made of iron (allowing for padding), which I padded well with cotton wadding, cut in strips and wrapped around the ring, over which I applied a bandage. To each side of the ring I sewed strips of bandage. I then placed a well-padded splint twenty-four inches long to the posterior aspect of the leg and thigh, which I secured by a few turns of bandage at the lower and upper ends, the bandage being loose so

as not to interfere with the circulation. I next brought the two fragments of bone into apposition, and placed the ring around the patella, and tied the strips of bandage over the splint, thus securely holding the ring in its place, and keeping the broken bone always in complete apposition, thereby giving the greatest possible chance for a bony union. At the expiration of thirty days, I removed the ring, and commenced passive motion of the limb, and I found the union to be bony and complete, and to-day (June 15th) he has very good use of the limb. During the time this case was under treatment, I invited Drs. E. H. Gregory, J. J. McDowell, E. A. Clark, E. S. Frazier, and other physicians, to examine the appliance, and they all expressed the opinion that it was *just the thing* for a fractured patella. The advantages of the ring in the treatment of this troublesome fracture are so apparent, that it seems to me hardly necessary to point them out. The appliance did not give the patient the least pain, and there was no interruption of the circulation by the bandages. It was impossible in this case for the patella to escape from the ring, but possibly in some cases, as of women, when there is a good deal of adipose tissue, and but little prominence of the patella, it may not be so easy to apply the ring; but I am persuaded that it will give entire satisfaction in all cases. I claim by the application of the ring to have reduced one of the ugliest fractures of the human frame to one of the simplest for treatment.

I report this case for the information of the profession, feeling confident from the perfect success of this case that the ring has never been used before, or we certainly would have been familiar with its use.

DR. MARION SIMS' SPECULUM SO MODIFIED AS TO MAKE IT SELF-RETAINING.

BY GEO. SYNG BRYANT, M. D., ST. LOUIS, MO.



The Speculum has now become so familiar in the treatment of uterine and vaginal diseases and injuries, that even the merest tyro in medicine is not wholly unacquainted with its use.

Since the revival of the speculum by Recamier, one of the physicians to the Hotel Dieu in Paris, numerous shapes have been invented, each variety having its advocate, claiming for it superiority over all others.

So badly constructed were the most of these different instruments for exposing to view and touch all the parts of the vagina, that little advance had been made in uterine surgery until Dr. Marion Sims, formerly of Alabama, brought forth by his inventive mind the peculiar speculum known as the "duck-bill," or Sims' Speculum.

This invention was made about twenty-one years ago, and since that time uterine surgery has made such progress that it is now regarded of the highest importance in the proper management of many diseases and accidents incident to women.

In vesico and recto-vaginal, and vesico-uterine fistules; in polypoids and fibroids of the os, cervix and cavity of the uterus; and in operations upon the os, cervix, and internal os, the Sims' Speculum was the only instrument which enabled the surgeon to get a proper view of the parts, and to manipulate with ease and certainty.

The only objection to Dr. Sims' Speculum that can be urged is, that an intelligent assistant is required to hold it in position during the entire time while it is introduced into the vagina. The services of an intelligent assistant can not always be readily had.

This great inconvenience led me to so modify the duck-bill blade as to make it self-retaining, and to be, at the same time, easily adjusted to any sized woman. This instrument consists of a blade *A* and lever *B*, with screws attached, so as to completely fit the lever to the sacrum and back, and to elevate or depress the blade at will.

This speculum operates precisely in the same manner that the Sim's Speculum does, with the exception that it retains itself. The blade *A* is introduced into the vagina, and then connected with the lever *B* by the notched joint *C*. At the end of the fenestra *H* in the lever, a strap is attached which is to be carried around the body or waist, and tied before the pad, upon which the button *G* rests, is to be placed upon the sacrum.

Above the bend of the lever is a slide *E*, to which is attached a screw with a button *G* at the end. This button rests upon the pad on the sacrum, and the screw is then turned until the requisite amount of tension is made upon the perineum.

The screw *D*, at the lower end of the lever, which rests upon the arm *I*, of the blade, is now turned until the free or distal end of the blade *K*, is elevated or depressed, as may be desired, to the proper degree to bring into view the os uteri and the whole of the vagina, except that portion upon which the back part of the blade rests. This speculum is easily fitted, and its introduction is without pain to the patient. It was made by Leslie & Co., of this city, from a diagram. The accompanying cut is only one-

third the size of the instrument. I deem it proper to state here, that at the time of submitting the diagram of my speculum to Profs. Pallen and Pope, of this city, I was not aware that any one had made the effort to so modify the "duck-bill speculum" as to make it self-retaining. Recently I have had the pleasure of seeing the self-retaining speculum of Dr. Emmet, of New York. It is a beautiful and ingenious instrument. I have used it, and also seen it used several times by Dr. Montrose A. Pallen. It does not do well in muscular women; but when the patient is lean and flaccid, with a thin and weak perineum, Dr. Emmet's speculum answers a good purpose. Another objection to it is, that it can not be well adjusted to different sized persons, and even when it fits the patient "the left lateral semiprone position" of Dr. Sims must be rigidly maintained, or displacement of the instrument will occur.

Correspondence.

EMPIRICAL PRACTICE—BERBERIS.

MESSRS. EDITORS:

On well-established principles, the profession is decided in its condemnation of empirical practice. This is well as a general rule. But it can not be denied that indulgence is occasionally given to the spirit of empiricism in almost all circles of the profession. In fact, no small share of medical science, as well as general science, can be traced back to a period when it was almost purely empirical. The time was when medicine was wholly an inductive science, and it has become in every sense deductive only since the labor of ages has collected enough facts to furnish material for generalization. No one would ever

have argued, *a priori*, that mercury, antimony, or Peruvian bark possessed any curative properties. The facts were reached by experiment, or by pure accident, and were none the less valuable from not having been reasoned out. Nor will any one assume that all the facts necessary to the perfection of science have yet been collected. The most illiterate boor may still report facts, the result of his experience or observation, which may be worthy of the attention of the most erudite.

Under this heading I desire to call attention to the fact, that from "time immemorial," the rural people of the old Southern States have almost uniformly treated all forms of "sore mouth" with the bark of the barberry. This I have known and observed for more than twenty-five years, and as it always seemed to answer the purpose, I have encouraged it without prescribing it. Since coming West I have heard nothing of it, and but for a recent experience in suffering, I might never have thought of it again. The past season in my vicinity was remarkable for the prevalence of sore mouth; and, in common with many others, I was for several weeks scarcely able to take any solid food. At times the power of articulation was so impaired, that I was able to speak only by great effort of will. After using all the remedies known to the profession to no purpose, or with no results, I chanced to recollect the old-time barberry, of which I had heard so much among the country people of North Carolina in former years, and resolved to try it. Accordingly, I procured a stick from a city garden, and commenced chewing the bark, or rather holding it in the mouth, and swallowing the juice; I also slept with a little of it in the mouth. On the second day the relief was very great, and on the third day my mouth was so nearly well that I thought of it only when my friends inquired concerning it. On the fourth day it was entirely well.

This, I presume, was simply an unusually severe case of *cancer aquaticus*, or *stomatitis*. Having some of the bark left, I distributed it among a few of my friends who were suffering from the same malady, and they all experienced the same relief as myself.

From this I began to have more confidence in the barberry than I had ever had before, and so resolved to seek opportunity for testing its efficacy in different forms of stomatitis. I soon found a woman who had been suffering for four months with stomatitis materna to such a degree that she was obliged to subsist wholly on liquid food. Though I had never heard of the barberry being used in this form of stomatitis, I resolved to administer it to her, seeing that her physicians had exhausted all means, and had abandoned her case. At first she was obliged to use it in the form of decoction, or infusion, so great was the ulceration. On the third day there was a marked improvement, and at the end of a fortnight she declared her mouth perfectly well.

These are simple facts; I throw them out for what they are worth. I have no interest in the matter more than any other person. I am fully aware of the fallacy of the old maxim, "*Post hoc; ergo, propter hoc.*" The relief might have come in all these cases without the treatment. So far as I know the treatment was purely empirical; I do not pretend to understand it. The bark is intensely bitter, and greatly excites the salivary glands. I presume it contains tannin. The alcoholic extract (berberin) is found occasionally in the drug stores, and is used by some physicians as a stomachic or tonic (see last edition of Dispensatory).

This plant must not be confounded with the Berberis Oxycantha Galeni, or pepperidge bush of England, though they are remarkably similar, and are thought by botanists to possess the same properties. The oxycantha was known to Galen as an antiscorbutic and tonic. Whether this has ever been imported to the United States I am not certainly informed. The plant which I have used is the Berberis Canadensis, or Berberis Vulgaris. It grows wild in Canada and in Massachusetts, and even as far south as Virginia, in the mountain districts. Further south it is found only in gardens. The berry has a pleasant sub-acid taste, and is much used for preserves, jellies, and tarts. The best mode of obtaining the bark is by scraping with a dull knife, which breaks it up and makes it soft. I think it best to carry it

in the mouth and swallow the juice, seeing that the disease is likely in some cases to extend into the æsophagus.

Begging the pardon of the profession for having thrust upon them so simple a matter, and hoping that it may not merit their entire contempt, I subscribe myself

A RETIRED PHYSICIAN.

Translations.

TREATMENT OF CHOLERA WITH SULPHATE OF COPPER.

"*Par le Dr. Lisle, Médecin en Chef de l'Asile des Aliénés de Marseille.*"

TRANSLATED FROM THE FRENCH BY R. H. HAYNES, M. D., ST. LOUIS.

The epidemic of cholera that has been desolating the city of Marseille for three months, has been more serious and more murderous than has been believed outside. It has been grave, especially in our asylum, where, out of a population of less than a thousand inmates, we have had, from the 29th of July to the 14th of October, 1865, sixty-eight well-characterized cases of cholera, and more than one hundred and fifty cases of "*cholerine*," gastric embarrassment, and other intestinal affections; the largest number of which would have produced the cholera if they had not been arrested at their commencement by an energetic treatment. The sixty-eight cases of cholera, forty of which were observed in men and twenty-eight in women, gave thirty-five deaths; seventeen of the first, and eighteen of the second.

The fourteen first patients submitted to my observation in the section for the men, were treated by the means the most generally accepted: alcoholic excitants, opiates, ice, frictions, and warm applications, etc.; some were even purged at the commencement. Out of this number, twelve died more or less rapidly; the other two had a most difficult convalescence, which was prolonged beyond six weeks.

This was little encouraging. I knew, moreover, that my brethren in the profession were but a little more fortunate. Nevertheless, what was to be done? I had indeed read in the journals of Marseille, two or three articles strongly recommending the preparations of copper, according to the ideas, rather theoretical than practical, of Dr. Burg. But as many others, I am distrustful of all the old or new remedies that come to us through the political journals. It was thus with me when your number of the 22d of last August, containing a long article from Dr. Burg upon the prophylactic virtues of the salts of copper, was communicated to me. Although this practitioner seemed not to have employed the salts of copper internally, and invoked his personal experience only for the treatment of the cramps by the external application of this metal, this became more serious, and I decided to try it.

The opportunity was not long in presenting itself. The 30th of last August, at six o'clock in the morning, a young, robust, and healthy woman, who had been in my service ever since my arrival in Marseille, was taken suddenly with the most serious symptoms: a very abundant rice-water, whitish, serous diarrhœa; a little while after, incoercible vomiting, and terrible cramps, nearly continual, and occupying all of the muscles of the inferior members and of the vertebral regions; pulse frequent, small, and afterwards thread-like; tongue moist and fresh, slightly bluish; burning thirst; the skin rather cool than warm; the eyes sunk back in their orbits, and surrounded by a brownish color (color of bistre). Treatment: Potion composed of 1 d. distilled peppermint water, 120 grammes; laudanum of Sydenham, 40 drops; 2 d. alcoholate of mint, 40 grammes; a litre of tea, with the addition of 150 grammes of rum; frictions and warm applications upon the skin; some pieces of ice from time to time.

In spite of this treatment the symptoms became rapidly more alarming, and at one o'clock everything seemed to indicate approaching death: the pulse was totally imperceptible; the face and tongue cyanosed and cold; the extremities were cold to the knees and to the elbows, and had become of a bluish tinge; the diarrhœa and vomiting continued and were no more voluntary; the cramps were all the time very painful. It was then, that, after having consulted with my colleague in charge of the service for women, who judged with me that death was imminent, I decided to administer to my patient, in a spoonful of sugar-water, at the same time two drops of laudanum, four drops of a solution of the sulphate of copper, prepared, as I then thought, according to the indications of Dr. Burg. One hour and a half after, an unhoped for change had been produced,—the cramps and vomiting had diminished in frequence and intensity; the expression of the

face was less anxious; warmth returned by degrees—especially the tongue and the breath were less cold; yet the pulse remained insensible, and the diarrhœa was still abundant. I prepared immediately a potion containing five drops of laudanum, and ten drops of the solution of the sulphate of copper, which was given at first by the spoonful, and then by the half-spoonful, every hour.

Towards evening the cramps ceased entirely, and soon after, the vomiting. The pulse and the warmth returned in the night; the diarrhœa, after having also diminished during the night, was nearly insignificant towards the morning of the second day. At last, although the thirst was still very great, the patient was doing so well that she spoke of eating. The potion was all taken and was not renewed.

Yet the period of reaction was laborious, and required an energetic treatment: *a bleeding the third day, leeches to the temples, two purgatives, and at last tonics.* It is useless to enter into longer details upon this subject. I add, nevertheless, that two days after the invasion of the disease, the patient was able to make a fatiguing voyage to her country, and that to day she is remarkably well.

This fact seemed to me sufficiently conclusive to stop all my hesitation, and since that day all of my cholera patients have been submitted to the same treatment. Yet I have made by degrees some modifications to the formula, which I shall make you acquainted with further on. I ought beforehand to submit some statistics to your observation, the exactitude of which I guarantee, and upon which I call the serious attention of your readers.

I have already said, that I have had, up to the present time, forty cases of cholera in my service, and I fear that it is not yet finished; the last died yesterday. You know what became of the fourteen first; the remaining twenty-six were submitted to the treatment by the sulphate of copper, and only five died. Twenty-one have then been cured, and with the largest number the convalescence has been prompt and of little length. The real disease has lasted but little more than twenty-four, forty-eight, or perhaps seventy-two hours, and the convalescence six, eight, ten, or fifteen days; and with the largest number the period of reaction has been null or altogether insignificant; and that which is worthy of remark, the principal symptoms—cramps, vomiting, cold, diarrhœa—have nearly constantly followed, in their diminution and successive disappearance, the same march as with the patient of whom I have just given the observation.

Still, I ought to insist upon an essential point: All these patients were as seriously affected as the first at the moment I saw them for the first time: all, with scarcely an exception, had abundant dejections from above and below, the matter of which was of the most characteristic; all had more or less violent cramps; in all except the last, the urine was suppressed during more than twenty-four hours, and in several for two or three days; all but

three or four offered evident traces of cyanosis; had the tongue and extremities cold to different degrees, the pulse small, thread-like, and in some altogether insensible.

In order to give all the worth to the preceding facts, I ought to say a few words about the five patients who died. The first was affected with general paralysis arrived at its last stage. He had been confined to his bed for two months by one of those diarrhœas that nothing can stop. It could be said that the cholera had really attacked but a corpse.

The second died in seven hours from one of those dry choleras, which according to the general saying, kill more promptly and more surely than the other. He was able to take but two spoonfuls of the potion.

Another was suddenly taken with such serious symptoms, that as soon as I saw him I thought all treatment would be useless. Nevertheless, I administered the sulphate of copper, which seemed for an instant to check the march of the disease; the vomiting, which was very frequent, ceased even entirely towards the third hour; but the other symptoms became rapidly more serious, and the patient died after nine hours of suffering.

It was entirely different with the fourth: this man seemed as seriously affected during the first hours as the preceding; the pulse was completely insensible, and remained so during at least eleven hours; yet a slow but constantly progressive amelioration was manifested under the action of the sulphate of copper; so well, that, after the twentieth hour of the invasion of the disease, the patient seemed to enter into a full reaction. This state lasted two days, and the patient had already taken a little broth and wine, when he fell into a state of excessive feebleness, followed by a profound coma, from out of which he could not be brought, either by frequently repeated sinapisms, or by a large blister applied to the epigastric region, which seemed very painful, nor at last by ten leeches applied to the temple. It is well to note that at the end of the third day he had two vomitings of a greenish matter, followed by distressing nausea. At the end of the fourth day, the patient had some convulsions, and died at the end of one of them. Whence came this persisting coma? Whence came the convulsions, which have never been noticed that I know in the ultimate period of cholera? I shall return to this a little further on.

The last and fifth, a young epileptic of a debilitated constitution, still more enfeebled by strong and frequent attacks, died yesterday, after four days of the disease, without having presented other symptoms of the cholera than frequent characteristic vomiting, and a general chilling, which after several alternations of diminution and increase, finished by carrying him off.

To the preceding numbers I should add a small number of women whom I treated with the sulphate of copper, and who were affected with cholera between the 6th of September and the 8th of October, during which time I alone was in charge of the two services; their number was six, including one of the sisters attached to the hospital, and the young girl of whom I have given the observation. Out of this number two alienated died, and in

both death came at the end of the fourth and fifth day, and was preceded by profound coma and convulsions, precisely as in the case that I observed in the section for men.

Now for a résumé: sixty-eight patients, men and women, have been attacked by the cholera since its invasion in the asylum up to the present day. Of this number thirty-six have been treated by the ordinary means, and have given twenty-eight deaths and eight cures: twenty-six men and six women, in all thirty-two, have been treated by the sulphate of copper; seven of these patients died, and twenty-five have been cured.

Here are the facts in all of their sincerity. I offer them with confidence to the reflections and appreciation of my medical brethren. Are they sufficiently numerous and conclusive enough to permit me to affirm that I have definitely resolved the problem of the cure of cholera? Will that which I have obtained in the midst of a population of alienated persons, whom I have constantly under my eyes, and whom I can visit at any hour of the day and night, be reproduced everywhere and always in the different conditions of hospital and civil practice? I do not know truly what to think of it. New experiments practiced upon a larger scale can only reply in a satisfactory manner to these difficult questions.

I then dare to hope that my appeal will be heard by the physicians of different countries that the epidemic can yet invade, and that before long the light will be made. * * * * * * * * * *

The solution that I have prepared is the following: Sulphate of copper, 5 grammes; distilled water, 100 grammes. Then with this solution I make a potion containing: Solution of the sulph. of copper at 1-20, 1 gramme, 50 cent.; laudanum of Sydenham, 10 drops; sugar water, 120 grammes.

This potion is given to the patient as soon as possible from the commencement of the disease, and to the exclusion of all other medication; in the severest cases by the teaspoonful every quarter of an hour; in milder cases, half of a tablespoonful ever half hour; and at last every hour in the mild cases. It is continued thus until the warmth returns to the skin and tongue, and the pulse is rising; then the dose is not given but every three or five hours, and it is discontinued as soon as the state of the patient allows us to hope that the period of algidity is terminated. But it is not doubtful but that the treatment can be modified according to circumstances. I should add, however, that for each successive dose I have never given more than the half of a tablespoonful, unless perhaps in the commencement of the most severe cases, where I gave a tablespoonful. * * * * *

Dr. Lisle here states that the drink he generally gives is warm tea with rum, in the proportion of 50 to 100 grammes of rum to a litre of tea.

He says that the quantity of copper absorbed varies from 20 to 23 centigrammes, and one or two of the patients have had

some symptoms of poisoning from the metal; and if indicated advises an emetic at the period of reaction, or the administration of iron by hydrogen.

All that we can say of the treatment employed by Dr. Lisle is, that it gives the best success that we have yet seen, and we think well worthy of a trial; and if others meet with the same success, we can well say that we have found a cure for this terrible disease.

Transactions of Societies.

PROCEEDINGS OF THE ST. LOUIS MEDICAL SOCIETY.

REPORTED BY R. S. ANDERSON, M. D., RECORDING SECRETARY.

[Continued.]

Dr. Linton, resuming his remarks, said that he had been studying and lecturing on the subject of Inflammation for nearly half of a century, but did not pretend to have mastered it. He repeated that he did not like the term "Inflammation," for if we use words to which no definite meaning is attached, we can not understand one another. There is a dispute concerning inflammation on this account. For example: surgeons do not admit a part to be inflamed if it is healing. He called this inflammation, for all diseases have a tendency to get well. Some will hardly admit inflammation until pus is formed, and some even until

mortification takes place. We do not misunderstand when we say—increased flow, hyperæmia, hypertrophy, suppuration, or mortification. We then understand one another. But not so when the term "inflammation" is used.

Physiology teaches us that the vital process consists in nutrition and waste. The tissues during life are undergoing incessant changes: one process is always disintegration, another nutrition; a progressive and retrogressive metamorphosis. This we all understand. The process sometimes changes; when brought to a halt it constitutes molecular death. There are other changes which constitute disease: whatever changes rapidly, or at all, this feeding and decay of parts; whatever excites or stimulates this process; anything producing changes above health, is an irritant. A cell is nourished at a certain rate; a little more than this constitutes irritation. His idea of inflammation, is increase in the process of nutrition and decay. Some will not admit this. There is a flux of blood, swelling, increase of temperature. Some do not call this inflammation. An exudation ensues; some then admit inflammation; others do not. Suppuration occurs, and then all admit it to be inflammation. It was inflammation from the commencement. This swelling of the cell is the commencement of inflammation. There is no doubt of this. If it goes far enough, there is suppuration. The ultimate changes of suppuration and mortification are not separate and distinct, but gradually shade into one another.

He held that the old axiom, "*ubi irritatio ibi fluxus*," is as good now as of old, and whatever determines this flow is an irritant. We have external and internal irritants. The microscope shows that the first effect of irritation is contraction of the vessels of the parts. But this temporary contraction is soon followed by dilatation, so that the vessels become passive in the process. When this active congestion takes place, he had been in the habit of attributing the result to the congestion.

It is the blood which nourishes the tissues, and also oxydizes them; so when the blood oxydizes a part, it also puts nutrition in its place. Is it not reasonable, then, to suppose that the blood,

the stimulator of these functions, when thrown in increased quantities, would produce increased action?

These inflammatory processes are very various, and there are a great many which he regarded as inflammatory, constituting nearly the totality of the pathology of the solids, tubercles, benign and malignant tumors, growths, etc. In order to be as concise as possible, he had reduced to aphorisms his opinions on this subject. He proceeded to read them as follows:

1st. Nutrition is a fundamental vital process, common to all organized beings.

2d. Whatever, when brought into contact with the organization, increases this process, is an irritant.

3d. All such agencies increase the flow of nutritious fluids to the parts affected.

4th. These fluids are determined to particular organs or parts; also, by exercise and repercussion.

5th. This fluxion is a *sine qui non* to the various processes called inflammatory.

6th. Hypertrophy is the initial step in these processes.

7th. Hyperplasia by cell proliferation, or free cell and fibre development in an exudation, comes next.

8th. A further step is the conversion of these products, or cells, into pus.

9th. These hyperplasias, or exudations, vary in their quantity and their quality, and constitute almost the totality of the so-called deposits and growths.

10th. When the cells (however generated) are of low vitality, and rapidly decay and die, they constitute tubercle; when they persist, accumulate, and grow, but tend finally to ulceration, they constitute the so-called cancers: when they are rapidly converted into fibrous tissue, then the growth or tumor is benign.

11th. These deposits, growths, and tumors, are then aplastic, explastic, and cacoplastic.

12th. The altered qualities of the neoplasms are dependent, in a great part, on previous changes in the fluids constituting the various diatheses, as the tuberculous, the cancerous, etc.

13th. There are no specific pathological cells, or other organic forms.

14th. All the heteroplasias, heterotopias, and heteromorphias, are but the normal tissues changed as to place, quantity, and quality.

15th. The indications of cure are the removal of irritating causes and other means of reducing the vascularity of the affected parts, and the correction of the diathesis or condition of the fluids.

Reviewing these propositions, Dr. Linton said that the first proposition needed no explanation, as its truth, from the lowest cryptogamic plant to man, is admitted by all. These changes go on in health and in disease.

Concerning the second proposition, thus: a blister, a scratch, a cut, a grain of sand in the eye, are irritants. Heat is an irritant, and its opposite, cold, is a sedative.

Dr. HAMMER asked, do we not find inflammation to be produced by cold?

Dr. L. said he would explain this under another head.

The third proposition is nothing but an enunciation of the old truth, "*ubi irritatio ibi fluxus*," no more. No one will dispute this. Every irritation produces flow of blood, whether white or red. It is a waste of time to explain this.

Concerning the fourth proposition, that this flow of fluid to particular parts is also by exercise and repercussion, he said that it included two propositions: 1st, exercise; and 2d, repercussion. No one will deny that exercise tends to increase the flow of blood; and over exercise, therefore, tends to inflammation. Too much study, care, etc., produce mania. As to repercussion, he would here answer the question of Dr. Hammer concerning cold, which acts in this way: Cold applied to a surface constringes the vessels of that part, and so tends to produce inflammation elsewhere. In this way pneumonias are produced, though not entirely, as the super-oxydation of the air in cold weather tends to the same result.

Dr. HAMMER said that he had been misunderstood. He had asked how cold causes an inflammation in the part to which it is applied, as in frost-bite, etc.

Dr. LINTON said, that is caused by reaction from exhaustion of the vessels of the part. Whatever brings on a flow, whether a direct irritant or not, produces inflammation. Cold does this indirectly. So repercussion by cold may produce increased determination to parts, and thus produce inflammation.

The fifth proposition states that there is no inflammation or degree of it without increased flow.

Dr. WATTERS said, the last degree of inflammation, gangrene, is no flow.

Dr. LINTON said, this is a *result* of inflammation, not a degree; the part dies by anæmia. He had hardly thought that this proposition would be disputed, though it had been. If he had said "increase of blood" he might have been incorrect, as he remembered the experiment of Virchow passing a thread through non-vascular, cellular parts, causing swelling of the cells; and also the appearance of hypertrophies, growths, etc., in parts having no blood vessels. But there is a fluid, white blood, which exists in all parts of the body. There is, therefore, no part of the body without vessels, not even the connective tissue. You can not find holes in the capillaries to let the blood out, but it does get out. If this process goes on we have then a flow of red blood. This swelling of the cell is the first stage of inflammation, and the increased flow is only additional.

The sixth proposition is, that hypertrophy is the initial step in these processes.

Dr. HAMMER asked what was Dr. L.'s definition of hypertrophy?

Dr. LINTON. Increase in the size of a part by increased nutrition. There may be also generation of additional elements. Muscular fibres not only get larger, but more of them are formed. There are, then, two kinds of hypertrophy—simple and numerical, or hyperplasia.

Concerning the seventh proposition, that hyperplasia is the next step in inflammation, he said: if we continue this irritation

the cells commence dividing. Virchow maintains that all the swelling of an inflamed part is the result of the swelling and multiplication of cells, and not of exudation. The old doctrine which he combats, is that an exudation takes place from the vessels, which is formless, with no cells or molecules, a hyaline blastima, and in this new forms are produced by fibrillation or spontaneous generation of cells. The old doctrine, he says, is not true. Therefore, in this proposition he had embraced both doctrines, and said that the hyperplasia is by cell proliferation, or by free cell formation in exudation, thus embracing both doctrines, as the truth in this matter is only a question of fact. He merely gave facts as given by authors, and did not condemn either doctrine, except that of spontaneous generation. There is nothing spontaneous, except God and the human will.

The next step in these processes (proposition eighth) is the conversion of these products into pus, probably by increased oxydation. This was as far as he had gone concerning the inflammatory processes.

The ninth proposition is, that these exudations or hyperplasias vary in their quantity and their quality. Both terms are again used so as to include both theories. They vary in quantity and also in quality, since they are both benign and malignant. They constitute almost the totality of the deposits and growth.

The tenth proposition is, concerning the formation of tubercle, cancer, and benign tumors. Cells, however generated, may form tubercle; that is to say, if an exudation takes place in the lung and cells are formed, they do not grow, but soon die, break up, become cheesy, and they form tubercles. These cells are diseased because of a diathesis—a disease of the blood. This is the explanation of the bad cells.

Dr. HAMMER asked if these tubercles were not sometimes formed in strong, stout, healthy men, and how would this be explained under the idea of a diathesis?

Dr. LINTON said that these might, perhaps, be euplastic tubercles. He did not contend that all tubercles were of the same structure. They may occur in a person but little disposed

to them, and he might not be as much diseased as others; healthy men might have them and live to an old age.

When these cells persist in growing and accumulating, they form cancers. Cancer may be said to be but a difference in the grade of the same diathesis; cancer and tubercle being kin to each other. Rokitansky speaks of cancerous tubercle, and tuberculous cancer. They may therefore be mixed. He did not now insist on this idea, but would, perhaps, at another time defend it. There is a lower vitality in these cancers than normal. When they reach the development of fibres they form benign tumors; when they ulcerate and die they form open cancers. Why do they not heal? The plasma of the blood is incapable of being transformed into cells and fibres, or the cells themselves have not life to be developed, and the patient dies from exhaustion, just as one may die of a diarrhœa. Another remark: when the diathesis is not so cancerous some of these products may be formed into fibres, thus constituting scirrhus; the other being cephaloid.

The eleventh proposition is, that these deposits, growths and tumors, are aplastic, cacoplastic, and euplastic. They are aplastic when they form tubercle; cacoplastic when they form cancer; and euplastic when they form benign tumors.

In the twelfth proposition it is maintained that the altered qualities of the neoplasms are dependent, in a great part, on previous changes in the fluids, characterizing the various diatheses. The form which these cells take is determined by the diathesis; there may be some secret change in the tissue itself. He had often thought that wounds would not heal in these states as kindly as in healthy subjects; wounds inflicted by operations in the last stages of cancer or consumption would not heal well. If this is the fact, it might be explained by the influence of the diathesis.

The thirteenth proposition is, that there are no specific pathological cells or other organic forms. This question was discussed years ago. He took this ground when it was maintained by others that there were separate cell forms for different pathological

tissues, benign and malignant, and that it was only necessary to use the microscope in order to determine the nature of these. He then maintained that there were natural cells existing in the body, and not coming from the outside, modified so slightly, if at all, as not to be discoverable by the microscope.

The fourteenth proposition is, that all the heteroplasias, heterologias, and heteromorphias, are but normal tissues, changed as to place, quantity, and quality. Some tumors are but an increased amount of cells; others but an increased amount of fat.

Concerning the treatment of inflammation, the fifteenth proposition, he had little to say. We all practice nearly alike, whatever may be our ideas of pathology. We have, for instance, an inflammation of the brain. The indication is to drive the blood away from the organ. This we may do by cold applications, bandages, bleeding,—not so much to reduce the general circulation as the local congestion. We use purgatives for the same purpose. There are some changes in the blood itself, and we give certain remedies to counteract these changes, mercurials, etc.

Dr. HAMMER, after some preliminary remarks, said that he would now consider the fifteen propositions presented by Dr. Linton. These were made up of a great deal of truth, and also of much fallacy. What was true in them was derived from Virchow, being twisted into apparent conformity to the gentleman's theory. What was not from Virchow was not true.

The first proposition, that "nutrition is a fundamental, vital process, common to all organized beings," is true. No one will dispute that.

Second proposition: "Whatever, when brought into contact with the organization, increases this process, is an irritant." Consequently, whatever brought into contact with the organization *diminishing* this process, is *not* an irritant; cold, for example, since it at once diminishes this process; consequently, it is not an irritant, according to Dr. Linton. He is lacking in this definition. Anything in or out of the system which is able to exercise an influence on the system, on the cells, deviating it

from the ordinary rule, is an irritant. Cold belongs to this class. Dr. L. says that cold produces inflammation *indirectly*. Cold applied to a part produces *hyperæmia* by repercussion, but it does not produce *inflammation*. These are two very different things which he confounds. Therefore the second proposition does not hold good as it stands, congestion not being inflammation; an irritant not being that which increases nutrition.

Third proposition: "All such agencies increase the flow of nutritious fluids to the parts affected." This is true, and is taken from Virchow. The old axiom, "ubi irritatio, ibi fluxus," meant "blood" only, and Dr. L. in his book uses this word. Here he is so wise as to substitute the phrase "nutritious fluid," thinking that it would be overlooked. This proposition is then true, but is not original with him, being taken from Virchow.

Fourth proposition: "These fluids are determined to particular organs or parts; also by exercise and repercussion." Here he again confounds two different things. Nutritious fluids are determined to parts by exercise; but the term "repercussion" has no application here, as it only applies to blood vessels, and not to nutritious fluids.

Fifth proposition: "This fluxion is a *sine qua non* to the various processes called inflammatory." If by "fluxion" is meant a flow of nutritive fluids, then he admitted the truth of this proposition, it being the doctrine of Virchow.

Sixth proposition: "Hypertrophy is the initial step in these processes." This is true or false, as you like. If you take hypertrophy in its common sense, to mean the result and end of a certain process, then it is false. If by hypertrophy is meant a swelling of particular cells, as stated by Virchow, then it is true.

Seventh proposition: "Hyperplasia, by cell proliferation, or by free cell and fibre development in an exudation, comes next." This, as it stands, of course, means nothing. It is not explicit, though it is from Virchow. It must be explained: Hyperplasia is the swelling of the cells; these may become prolific, and from them new cells may be formed. If this is what is meant by "hyperplasia," it is correct. But before this takes place there

is an intermediate step. The first stage takes place; there is a flow of fluid and a swelling of the cells. Now, these swelled cells may lose their fluid and be restored, or may be broken down by albuminous degeneration. These are steps in the process. This is, then, an omission. The proposition may therefore stand with this explanation of hyperplasia, and the understanding of those other steps.

He had failed before to notice that Dr. Linton admitted free cell and free fibre development. Free cell development in physiology has been abolished. Whatever holds good in normal tissues is also true in pathological tissues. Then, since it is proven that in the normal tissues cells are only produced from other cells, it is also proven in disease. Dr. Linton's example, of the production of cells by the mixture of albumen and oil, is too absurd to combat. You can no more make a single cell than you can a man.

Eighth proposition: "A further step is the conversion of these products, or cells, into pus." This is by no means true. Admitting the general truth of the proposition, there are a number of intermediate steps. These cells may take many other forms than pus cells. In an inflammation of a mucous membrane, the microscope discovers no pus cells, but young epithelial cells, formed from the old swelled cells of the part. Dr. Linton says that tubercle is a result of inflammation, the cells being diseased, degenerating, etc. This, then, is another step in inflammation before pus is formed. Were this proposition true, then the discharge from the nose at every cold would be pus. Therefore the eighth proposition does not hold good.

Ninth proposition: "These hyperplasias or exudations vary in their quantity and their quality, and constitute almost the totality of the so-called deposits and growths." As to "exudation," he uses this term in reference to free cell development. But there is no free cell development. The exudation is therefore irrelevant entirely to the process. Hyperplasia and exudation are different, and can not be reconciled. As to variation in "quantity and quality," we let this pass; the *hyperplasia* may vary in these, but *not the blood*.

Tenth proposition: "When the cells, however generated, are of low vitality, and rapidly decay and die, they constitute tubercle; when they persist, accumulate, and grow, but tend finally to ulceration, they constitute the so-called cancers; when they are rapidly converted into fibrous tissue, then the growth or tumor is benign." Now this is partly true and partly false. When cells are of low vitality, that is, under certain circumstances which we do not understand, they do not grow or develop, but perish by true caseous degeneration, and constitute tubercles. What is commonly called tubercle is not so. The true tubercular cell is what is called the miliary tubercle,—small numbers of cells collected together, afterwards decaying into cavities, etc. The second part of the proposition is entirely false, because we have a great many growths which persist, grow, and tend to ulceration which is not cancerous; therefore this criterion is not a good one. The third part of the proposition is, that "when they are rapidly converted into fibrous tissue, then the growth or tumor is benign." Therefore all fibrous tumors are benign; consequently, all other tumors are malignant,—the fibrous tumor being the only exception. This is as fallacious as possible, as will be hereafter shown.

Eleventh proposition: "These deposits, growths, and tumors, are then aplastic, cacoplastic, and euplastic." What is "aplastic?" No cell formation at all. There then remains cacoplastic and euplastic. The fibrous tumors are, then, according to Dr. Linton, euplastic, and all other tumors are cacoplastic. He had no objection to these names, only they savor too much of humoral pathology.

Twelfth proposition: "The altered qualities of the neoplasms are dependent, in a great part, on previous changes in the fluids, characterizing the various diatheses." The words, "in a great part," are added to the original paper as read by Dr. Linton. Against this he protested. Leaving this out we have here expressed the humoral pathology; all diseases originating in the blood. This he has *not* taken from Virchow. All changes in the system are caused by previous changes in the blood. In

inflammation in one tissue, only epithelial cells are formed; in another, only pus cells; in another, cartilage, etc.: all this, he says, comes from the blood, just as though they were the secretion of the blood. Now, common observation shows at once that this is not so. In an inflammation of the epithelial membrane the product is almost always epithelial cells. The connective tissue has always a tendency in inflammation to produce pus. In the inflammation of a muscle the suppuration is from the connective tissue of a muscle, and not from the muscle itself. This tissue, then, has a peculiar tendency to produce pus. Each tissue, therefore, forms its own products of inflammation. These things are only to be learned from the microscope.

Concerning the diatheses, we will only consider one,—the cancerous. This is said to depend on a diathesis. Simon goes so far as to assert that cancer is an artificial means of eliminating a poison from the system, and that it is the worst practice to cut them out. No practitioner of the present day would act on this theory; on the one hand, we know that persons suffering from this disease have invariably died when unoperated; and, on the other hand, cases are related where open cancers have been repeatedly operated, have repeatedly relapsed, and have been finally cured. Dr. Linton asks, how is it that extirpated cancers will not heal? They do always heal, but new ones are formed in their places. He says that the bad plasma of the blood prevents their healing. This is not true: in the first place, because they *do* heal; in the second place, the relapse may be explained in this way. Ottoway says that there is always a certain area of tissue around the cancer which is infected by it; the cells of this area are modified by the influence of the cells of the cancer, and for two or four inches around, the tissues are not healthy, and all the elements in this areola are so changed as to be able to reproduce the cancer after the healing; therefore he advises to cut wide of the diseased part. But even then we can not always succeed, for the cancerous matters may be diffused through the system by means of the lymphatics and other agents, and may reproduce the disease in other parts.

Cancer is now looked upon by nearly all eminent pathologists as a local disease. There is no such thing as a primary blood diathesis. Take, for instance, the inflammatory diathesis, depending on super-fibrination of the blood. We now know that no such thing exists except as the consequence of some change in a particular organ. It is therefore not primary. So it is exactly the case with cancer. This is local. Why? Because we can not understand a discrasia existing for a life time, the blood independently remaining in a diseased condition. Now, let us suppose a cancer, localized in this or that organ, depending on a discrasia of the blood. Could such a disease heal? No. The source is still existing. No chemist or microscopist has yet found either evidence of or remedy for this discrasia of the blood; therefore it must multiply and prove fatal. But we see that it does heal. Besides holding the doctrine of discrasia, we are compelled to adopt the practice recommended by John Simon, who held that the appearance of cancer is but the effort of nature to eliminate the poison from the system, and therefore should not be interfered with. What does *our* observation show? When cancer is let alone death is the result, with very few exceptions. On the contrary, we know by experience, that if we interfere at the proper time, by the knife, cautery, etc., we can cure the disease. This can not be denied, as the statement can be sustained by the highest authorities. It might be said that the cancerous diathesis had given away, and in consequence the patient had recovered. But this would be but poor logic.

We have studied and know by what means a cancerous diathesis may secondarily be brought about. After a cancer has existed for some time, it is multiplied. But we can trace the means of this propagation. We know that it is partly by continuity, diseased matters being carried by vessels contained in malignant growths to different parts, and there giving rise to similar troubles. We can follow this in the case of a young lady with cancer of the scapula. She appeared to be the picture of health. The scapula was removed; after some time the disease appeared in the supra-scapula region; being removed from that part it

again appeared in the spine, and caused the death of the patient. The organs by which this propagation generally takes place are the lymphatics, then the blood vessels. Fibrous and nervous tissue may also be the means. Dr. Hammer then read the following extract from Virchow's Pathology (p. 161, bottom of page, lecture vi.):

"If the blood be considered as a whole, in contradistinction to other parts, the most dangerous thing we can do is to assume what has at all times created the greatest confusion; namely, that we have in it to deal with a fluid in itself independent, but upon which the great mass of tissues more or less depend. The greater number of humoro-pathological doctrines are based upon the supposition that certain changes have taken place in the blood, more or less persistent; and just in the very instance where these doctrines have practically exercised the greatest influence in the theory—namely, of chronic discrasia—it is usually conceived that the change is continuous, and that by inheritance peculiar alterations in the blood may be transmitted from generation to generation, and be perpetuated. This is, I think, the fundamental mistake of the humoralists,—the real hinge upon which their errors turn. Not that I doubt at all that a change in the composition of the blood may pertinaciously continue, or that it may propagate itself from generation to generation; but I do not believe that it can be propagated *in the blood itself* and there persist, and that the blood is the real seat of the discrasia. My cellulo-pathological views differ from the humoro-pathological ones essentially in this, that I do not regard the blood as a permanent tissue, in itself independent, regenerating, and propagating itself out of itself, but as in a state of constant dependence upon other parts. We need only apply the same conclusions, which are universally admitted to be true, as regards the dependence of the blood upon the absorption of new nutritive matters from the stomach, to the tissues of the body themselves also. When the drunkard's discrasia is spoken of, nobody of course imagines that every one who has once been drunk labors under a permanent alcoholic discrasia; but the common opinion is, that when continually fresh quantities of alcohol are ingested, continually fresh changes also declare themselves in the blood; so that its altered state must continue as long as the supply of fresh noxious matters takes place; or as, in consequence of a previous supply, individual organs remain in a diseased condition. If no more alcohol be ingested,—if the organs which had been injured by the previous indulgence in it be restored to their normal condition,—there is no doubt but that the discrasia will therewith terminate. This example applied to the history of all remaining discrasiæ, elucidates in a very simple manner the proposition, that *every discrasia is dependent upon a permanent supply of noxious ingredients from certain sources.* As a continual ingestion of injurious articles of food is capable of producing a 'permanently faulty composition of the blood; in like manner persistent disease in a definite organ is able to furnish the blood with a continued supply of morbid materials.'"

The great point in this is, that though contamination may exist, it does not depend on a disease of the blood itself, but on diseased matters introduced by the organs involved.

It is necessary in some questions to go further back than our own existence. In the embryo, at a certain period of its existence, there is no blood. Certain diseases may be transmitted from parent to child. A syphilitic father, the mother being healthy, produces a syphilitic child. Now at an early stage of the embryo we have to deal only with cells. The semen consists of cells and inter-cellular substance alone. Could the disease be communicated by the blood of the father when there is no blood either in the semen or in the child? As the cells of the child become developed blood is formed, and the syphilitic poison from the cells of the child is taken into the blood, and contamination results. Therefore, he thought, Dr. Linton very unhappy in upholding this theory of discrasia of the blood.

The thirteenth proposition is, "that there are no specific pathological cells or other organic forms." There are no new formations, but there are cells found in malignant growths which are not found in the natural body after birth. In any one of these growths we find the same cells as in the healthy body, but in addition to these we find others which are otherwise contained only in fœtal life. These are therefore different from natural. When a peculiar cell of embryonic life—that found in the bladder of the fœtus—is found in a growth of the adult body, we have no doubt that the growth is malignant. The cell is heterochronic; it differs in time from those of the normal tissues. We find cells also contained in the marrow of bones which are not natural to born persons. Virchow, in his latest work, gives distinctly the marks between cancerous and non-cancerous growths. It is also given in his work on Pathology. Cells of these kinds may exist in growths not cancerous; but not only cancers are malignant, but others also. The medullary sarcoma is more malignant than cancer itself. When the tissues are arranged in such a manner as to leave interspaces containing these peculiar cells, the growth is malignant. The diagnosis depends not only

on these cells in conection with tissue cells, but also on the peculiar structure and arrangement of the tissues.

The fourteenth proposition is, that "all the heteroplasias, heterologias, and heteromorphias are but the normal tissues changed as to place, quantity, and quality." He did not notice this further than to say, that in this proposition Dr. Linton acknowledges his errors in his former articles.

[To be continued.]

REPORT OF A CASE OF COMPOUND COMPLICATED FRACTURE OF THE LEG.

Read before the Morgan County Medical Society, at Jacksonville, Illinois, August 9th, 1866.

BY WM. S. EDGAR, M. D.

Communicated to the St. Louis Medical and Surgical Journal for publication, by request of the Society.

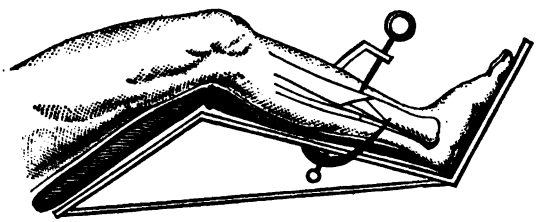
GENTLEMEN:—In compliance with the request of the Society at a previous meeting, I propose to read my notes of a case of compound complicated fracture of the leg, happily illustrating the great value of Malgaigne's spike in the treatment of oblique fractures, where the integuments do not admit of lateral splints to preserve coaptation; as well as where the fracture is too near the lower extremity of the tibia to admit of extension, for which cases it was originally introduced by Malgaigne, in 1843; also in cases of ununited fracture, as recommended by Dr. David Prince, in his cases, published in the Transactions of the Illinois State Medical Society for 1863, and in the October number of the American Journal of Medical Sciences for the same year.

On the 8th day of December, 1865, I was requested to visit Mr. D., distant nine or ten miles in the country, with his family physician, Dr. Weagley. Having learned from the messenger some particulars concerning the patient, I took with me such appliances as were likely to be needed; also my son, a medical student to assist.

I found the patient to be a man about forty-five years of age; rather tall, slender, and loosely connected; had been a soldier in the late war, but discharged on surgeon's certificate of disability on account of *varicose veins* of the right leg, which member had remained weak and swollen, more or less, since his discharge.

I found the man suffering from wounds of the right leg, produced by the kick of a horse, as he was falling forward upon his team from a load of wood.

On inspection of the limb both bones were found to be broken, the blow having been received on the outer aspect of the limb over the fibula, producing a wound in the integument, but not immediately over the fracture of that bone; also much tumefaction and ecchymosis. The point of fracture was at the junction of lower and middle third of the limb. Opposite this point the tibia was also fractured obliquely from above downward and outward, the upper fragment being forced through the integuments, producing a wound over the seat of fracture; crepitus could also be perceived in and downward from the wound, but whether it extended obliquely across the shaft, or longitudinally down the center, could not be certainly ascertained.



The drawing will illustrate better than language can the nature of the injury. The obliquity of the fracture facilitated shortening of the limb on contraction of the muscles.

The leg was much swollen from the knee down, and knotty from the remains of varicose veins, with an open wound over the fracture: hence, properly denominated a compound complicated fracture, which, in military practice, would have been condemned to amputation; but as the patient was where he could be well cared for, we determined on an effort to save the limb.

The immediate and pressing danger was destructive inflammation of the integuments from so extensive injury of parts impaired by previous disease.

To support the limb and secure coaptation of all the fragments, I molded a piece of sheet gutta-percha (of proper size) to the under side of the sound limb. Before applying the rubber, softened with hot water to mold it, the surface should be shaved, or the rubber covered with thin cambric to prevent its adhering to the hairs. After this it was well padded and overlaid with several thicknesses of coarse, open cotton cloth, which, by separating the splint from the limb, favored evaporation; the obstruction to which is the chief objection to the gutta-percha splint. The patient was now etherized; his injuries carefully examined; the limb extended and placed in the splint, which being slightly elevated on a straw pillow, was left without other dressing, except the application of cold water; an anoydne was directed to be repeated as occasion might require.

On the 10th, two days after the injury, I found the patient complaining of much pain in the limb, which had shortened some since being placed in the splint. The water dressing was continued, and the anodyne increased to secure sleep.

On the 11th, finding extension necessary to prevent shortening, I placed the gutta-percha splint on the double inclined plane, with foot piece and screw attached, applying adhesive straps to either side of the limb below the fracture, which surface was, however, too limited to depend upon for extension without the assistance of a broad roller crossed over the instep. This and the cords through the loops of adhesive plaster were secured to the footpiece. After elevating the knee properly, the foot was sufficiently extended by the screw to secure coaptation. This might

have been done at first, but for the necessity of leaving the limb as free as possible until the circulation should be improved.

The wounds over the fractures were supported by adhesive plaster, and the cold water dressing continued.

I next saw the patient on the 13th, when the swelling and ecchymosis were subsiding, and he was generally more comfortable; treatment continued until the 19th, eleven days after the injury, when I again saw the patient, and found the wound over the fibula quite healed; the general aspect of the limb much improved, but excoriation about the heel complained of, which I tried to relieve by padding above; but finding this ineffectual, on the 22d, I removed the gutta-percha splint, believing the moisture with which the surface under the splint was constantly bathed to favor the excoriation; a wood splint well padded, with free space under the heel, was now substituted.

The wound in the integuments over the tibia was suppurating, a part of the discharge coming from the fracture, which seemed disinclined to remain in close coaptation, the upper fragment wandering from its fellow out through the wound; to obviate which, a splint was carefully molded to the anterior and inner aspect of the limb, which being cut away opposite the open wound and padded liberally, was applied with gentle pressure over the tibia, with the hope that it would steady and retain in coaptation the broken fragments of the bone.

On the 29th I saw the patient again: the fibula seemed supported by callus, and doing well; but on removing the dressing from the tibia, the wound was found to have extended under the splint, and the bone was still disposed to project, notwithstanding the limb had not shortened; also a slough had commenced over the upper point of the lower fragment in front of the limb.

It was now evident that the soft parts would not bear the pressure or confinement necessary to retain in place the broken fragments; consequently, all dressings were removed; the extension slightly increased by attaching a weight to the foot-piece; the wound dressed with solution of permanganate of potash—

which, it is hardly necessary to state here, is an admirable dressing for all ill-conditioned ulcers, correcting the peculiarly unpleasant fetor from suppurating bone instantly, and stimulating healthy granulations,—the strength being varied to suit the circumstances; from three to thirty grammes to the ounce of water.

Thus baffled in every effort to retain in apposition the fracture of the tibia, I left the patient to nature until I could cast about for new appliances to meet the exigencies of the case; *i. e.*, secure coaptation without pressure on the soft parts.

I fortunately remembered Dr. Prince's case of ununited fracture, alluded to elsewhere, and through the politeness of Dr. Bibb (Dr. Prince being absent), procured the use of the spike and clasp originally used by him on that case.

Accordingly, on the 1st day of January, I visited the patient, and applied the steel spike about an inch from the end of the upper and overlapping fragment through the open wound, by which means the bones were readily brought into perfect and close coaptation without disturbance of the soft parts or pain to the patient, after the effect of its barbarous appearance was over. The following day I visited the patient to make sure of the success of the new appliance. On the 6th of January I again visited the patient, and found the end of the clasp which rested against the splint, on the opposite side of the limb from the fracture, had slipped, and slightly disturbed the bolt; also the open wound through which the bolt reached the bone gradually enlarged, and seemed irritated by the iron. As the weather was very cold, it might have been from reduction of temperature, the iron bolt being a good conductor.

I therefore determined to have a new instrument made, with an additional screw to confine the end of the clasp to the splint, and thus steady the instrument; also, to substitute *ivory* for the steel bolt, appreciating Dr. Prince's statement (on the seventieth page of Illinois State Medical Transactions for 1863), "that the reason why this treatment has not been adopted before this time, is, probably, the repulsive appearance of the treatment to the patient and his friends." Believing the ivory bolt would look

less formidable, and answer a better purpose, on the 12th of January I substituted the new instrument, finding but slight change in my patient since the last visit, except that the excoriation about the heel rendered the patient very uncomfortable. Believing the union of the fibula at this time sufficient to act as a splint in keeping the limb extended, I removed the extension upon the foot to the great relief of the patient, and depended on the spike to preserve coaptation of the bones, as exhibited in the drawing.

On the 21st of January I again saw the patient, and found the wound discharging healthy pus and granulating.

On the 26th the bones were still held firmly together by the bolt; the patient seemed doing well; slight exfoliation of bone had taken place through the open wound.

On the 30th the wound had healed close around the spike, but the heel was still painful. All dressings were now removed from the limb, and it was lifted clear from the splint and washed in its whole length; although the fracture seemed firm, I determined to let the bolt remain another week.

Concerning the ivory bolt, it was apprehended that there might be decomposition of its animal matter, and thus prove a source of irritation to the wound, which in this case was not realized. Its advantages over the iron bolt seem to be, that it occasions less irritation to the soft parts, and less exfoliation of bone; it is lighter and less formidable to the patient and his friends, while it has all the service of the steel spike.

On the 7th of February, five weeks after the application of the spike, I ventured to remove it. The sores which had been occasioned by the weight of the limb under the fibula, and those occasioned by extension other than at the heel, had disappeared; patient's general health was good, a nutritious diet of animal food being indulged. The excoriation about the heel alone interfered with the patient's comfort, to relieve which a broad piece of adhesive plaster, about eighteen inches long, was applied to the sole of the foot, vertically reaching a little back, but short of the excoriated surface. This was secured to the sole of the foot by

cross strips and roller, the upper end being carried over the foot-piece and secured to the extension screw. In this way the foot was *suspended*, to the great relief of the patient; the excoriation now healed rapidly.

On my visit a week after, the opening through the integuments left by the bolt had closed, and the limb was found to be symmetrical and of proper length. The swelling had so far subsided as to enable me to distinctly trace the tibia, which seemed to have made proper union. The limb was supported as usual by the many-tailed bandage, until the 25th, when a *starch* bandage was applied, and a gore removed from it over the muscular portion of the leg, and the bandage supported by a roller readjusted from time to time, as compound fractures unite by the slow process of granulation. Erichson says: "In a compound fracture union takes place by the ends of the bones which lie bathed in the pus of the wound granulating and throwing out plastic matter, which becomes directly converted into bone, their being in many cases but little provisional callus. The union of these fractures precisely resembles that of a wound in the soft structures by granulation, the process occupying a much longer time than that required for union of simple fractures."

The starch bandage was worn a month longer to secure a good degree of consolidation before the limb was finally turned out. I have seen the limb once since; it was still considerably swollen, but looked healthy and free from varicose veins, and gives fair promise of being a more useful member than it was for several years before the accident.

In conclusion, gentlemen, you will indulge me in stating, that in my experience with the use of gutta-percha in the treatment of fractures (which has not been extensive), I find it chiefly valuable in giving support to fractures requiring the splint to be accurately molded to the parts, as on the palmar side of the fore arm, lower jaw, over the malleoli, etc.; but the impediment it interposes to evaporation from the surface it may cover, is a serious obstacle to its general use.

Its great utility in compound fractures is, that it does not absorb the purulent secretions which may flow over it, and hence it may be perfectly cleansed from time to time.

In the case to which your attention has been invited above, I think you will join me in the opinion, that without the help of the spike, osseous union would have been improbable; and if effected at all, it would have been attended with such deformity as to render amputation preferable.

Dr. PRINCE followed with remarks further explanatory of the treatment.

The following case, reported by Dr. Prince, will be of interest, as it is believed to be the first instance in which the spike has been applied to the femur:

Mrs. McGinly, a small Irish woman, aged about thirty (having had three children), at Ashland, Cass county, Illinois, received an oblique compound fracture of the middle third of the femur, January 16, 1865. The direction of the fracture was from within, outward and downward.

Dr. JACKSON applied a long splint, and after five weeks, substituted a paste-board splint, which was worn some weeks longer.

July 13th, 1865—six months from the date of the injury: There is an external projection of the lower end of the upper fragment, which is adherent to the cicatricial closure of the original wound. There is limited motion antero-posteriorly, but none laterally. The limb is two and a half inches shorter than the other, measuring from the superior anterior process of the ilium to the internal malleolus of the tibia; but measuring on the outer side of the thigh, from the top of the trochanter to the head of the fibula, carrying the tape line over the convexity of the projection, there is no difference in length on the two sides.

Force was employed under ether to break up the soft callus and restore the fragments to their proper relations. A long splint was applied, to which was nailed a back piece, extending from the ischium to the foot-piece. A hole was cut out to prevent the possibility of injury to the heel. Extension was made by adhesive plaster, and counter-extension by a perineal band.

The modification of Malgaigne's spike, shown in the figure, was then applied, so that the sharp point of the spike came upon the lower projecting end of the upper fragment, while the hook passed over or anterior to the thigh and under the board, constituting the posterior portion of the splint. This held the upper fragment in apposition with the lower.

The case was left in charge of Dr. B. C. Randall, of Ashland, who wrote, of date October 20th, 1865, three months from the time the spike was applied, and nine months from the time of the injury: "She has completely recovered the use of her limb. She does not bear as much weight upon it as she could, more from timidity than any thing else. She will gradually get more confidence, and I think by the first of December, will be able to walk any reasonable distance. I removed the spike September 20th (after two months' application); the wound healed kindly and quickly, without any spiculæ of bone coming out. She used morphia three or four days after the operation, after which the pain and irritation were so slight that its use was discontinued.

Measurements.—Fractured side: From sup. ant. sp. process to int. malleolus, 29 $\frac{6}{16}$ inches; from trochanter to upper extremity of fibula, 14 inches.

Sound side: From sup. ant. sp. process to internal malleolus, 30 $\frac{10}{16}$ inches; from trochanter to upper extremity of fibula, 15 $\frac{4}{16}$ inches. So you see there is one inch and a quarter difference produced by overlapping of the fragments of the femur. I was very particular about keeping up the extension."

Since the date of this letter it is understood that the patient executes her domestic labor and goes where she pleases, without impediment from the limb.

This expedient must be claimed to be a great acquisition to surgery. It is by no means probable that the fragments could have been made to approach sufficiently close to unite by any other external appliance.

Review of Current Medical Literature.

SURGERY.

1.—*A New Curative Method against Cancer.* By Prof. C. THIERSCH, of Erlangen. (*Bayer. ærztl. Intelligenz-Blatt*, 1866, April 23.)

Prof. NUSSBAUM, of Munich, communicates to the above journal a private letter of Prof. THIERSCH, upon a subject which appears to merit the most serious attention; we propose to extract from the article at some length. Prof. THIERSCH says:

Within the last weeks I have applied a new method of treatment to a malign tumor, which is based upon the principle of bringing medicinal agents into immediate, universal, and continuous contact with the humors and tissues of the growth, in a dose which causes neither inflammation nor gangrene. It is true, the experiment so far has led to no definite result, and will probably not do so in the present case, because the life of the patient under treatment is endangered in the extreme by the supervention of pyæmia; but such remarkable changes have occurred in the status of the growth, that I venture to express the hope that we may succeed, in the way proposed, to exert an immediate influence upon the nutrition of many growths.

The case is briefly as follows: A farmer's wife, fifty-six years old, presented herself at the surgical clinic in Erlangen, with an ulcerating growth of the size of a fist occupying the right half of the face. One and a half years ago, the patient was struck in the right temple by a cow; without any immediate swelling, a small painless node formed about three months afterwards; in the summer of 1865 it grew as large as a hen's egg, and in the course of the fall it attained the size of a fist; at first the skin was movable over the tumor, afterwards it became adherent, and the tumor had a nodulated appearance. At this period the tumor had been repeatedly cauterized, the slough removed, and thus the tumor reduced in height, but without checking the extension of the degeneration. In January, 1866, this treatment was inter-

rupted; six or seven weeks later, the patient entered the above clinic, the tumor having already attained more than its former size. The degeneration presents a nearly circular surface, seventeen centimeters in diameter, extending anteriorly into the frontal skin, the external third of the eyelids and the cheek, downward to the angle of the jaw, partly filling the upper part of the carotid triangle, and covering the contours of the insertion of the sterno-mastoid muscle which is involved in the degeneration; posteriorly the tumor embraces the meatus auditorius. The centre of the growth consists of a group of large, crowded, ulcerating tuberosities; at this place the tumor measures about five centimeters in depth. The margin consists of a continuous row of nodes, forming a wall around the whole; between this and the central elevation, a kind of circular sulcus is found, two to three centim. in width. The ulcerated surfaces are uneven, covered with coarse granulations; the secretion is copious, discolored, and in a short time infects the atmosphere of the spacious ward with the peculiar odor of cancerous ichor. The general health of the patient is comparatively good; she complains of pain in the affected portion of the sterno-mastoid, and in the ear, and is somewhat emaciated, but free from fever.

Not expecting even a temporary success from extirpation or cauterization, the author felt justified in making an experiment which had occupied his mind for some time. He had already expressed, in his work on epithelial cancer, the opinion that we need not despair of finding a mode of curing cancer if we succeeded in applying the proper remedies for some length of time directly to the prolific tissue-elements themselves, in order to check their proliferation without killing them; and that subcutaneous parenchymatous injection might be adapted to this purpose. The only fear was that the remedy might be too rapidly absorbed, and that too frequent repetition of the injection, by its traumatic effect, might rather increase than diminish the rapidity of growth. For this reason he rejected preparations of iodine, and chose nitrate of silver, which we know to have an affinity for the tissues. In the preparation of microscopic objects,

if thin sections of hardened tissues are soaked in a solution of nitrate of silver, 1 to 50,000, or even 100,000, exposure to sunlight will color the objects by reduction of the silver. It may be presumed that the cells of living tissues also are capable of reacting upon silver-salts with which they are brought in contact; the silver is probably converted into a chloride,—a circumstance which seems desirable, because the latter is little soluble in the circulating fluids. To prevent extensive coagulations of albuminates by the silver, Thiersch resolved to follow up the injection of silver by that of a solution of common salt, which would partially redissolve the silver-albuminates, and convert the remaining nitrate into the chloride which would better resist absorption. His plan, therefore, was to charge the degenerated parts and their vicinity with a solution of silver, to favor the formation of the chloride by injecting common salt afterwards, and to watch the result. The dose for the first injection was chosen at 1:5000 for the solution of silver, and 1:2500 for the chloride of sodium.

The first injection was made on the 13th of March. The canula of the syringe was introduced into the healthy skin, one and a half to two centim. from the palpable margin of the tumor, and pushed completely into the growth in the direction towards the centre; one third of the contents (the syringe holding one cubic centimeter) was expelled, the canula withdrawn one-third its length, and the second third injected, and lastly the third portion injected into the healthy vicinity. This process was repeated at intervals of from three to four centim. around the entire circumference of the growth, about twenty times. After these injections—about ten minutes after the beginning of the operation—the same quantity of the solution of common salt was injected in the same way, choosing points intermediate between the previous punctures. During the operation a little of the injected fluid was sometimes seen to exude from the ulcerated surface, yet not much of it was thus lost. The uniform intumescence of the whole mass, and the œdematous swelling of the vicinity, proved that the first object—the uniform diffusion of the injected fluid in the parenchyma—had been accomplished.

On the following day, the œdema of the neighborhood had increased a little, but the watery swelling of the tumor itself, which the injection had caused, had not only disappeared, but a part of the rampart-like margin upon the forehead had become lower and felt firmer than before; and on the 15th, this sinking and hardening of the marginal portion had progressed. The second injection was now made with a solution of silver 1:2000, and a solution of salt 1:1000. Twenty-four hours after this operation, the growth has become considerably smaller; the surface of the margin is no longer globular, smooth, shining, tense, but somewhat levelled, nodular; the skin a little wrinkled. On the ulcerated surface several very small sloughs have formed; otherwise it has a good appearance and copious secretion, and no longer diffuses the same terrible odor as before. "This considerable change in the growth, consisting chiefly in diminution and condensation, having occurred in so short a time, could only be owing to an extensive resorption of parenchymatous fluid. Hence, an essential change had certainly occurred in the power of imbibition (*Quellung*) of the degenerated tissue. Whether this was caused by a direct alteration of the fluid, or by primary action upon the tissues, or by both together, I must leave undecided. The next days would show whether this draining, if I may so speak, would be followed by disintegration and absorption of the cellular elements."

On the 17th the diminution continued; the vertical diameter is reduced from seventeen to thirteen centimeters, the transverse diameter from seventeen to fourteen, and the height of the tumor by one centimeter. No inflammatory reaction having taken place around the punctures, the solution of silver for the next injection was chosen at 1:1000. But this dose proved too large. The injection caused a much greater œdematous swelling which became red and painful, and deep-seated suppuration in several places. Nevertheless, the diminution of the tumor progressed wonderfully. The margins are level; the ulcer is covered with tough, healthy-looking granulations; no longer any cancerous odor. The vertical diameter is now (March 21st) only ten centim.,

the transverse only seven. The success, so far, was a most agreeable surprise. The tumor having become so dense that the absorption of fluid could amount to but a very inconsiderable part of the diminution, the latter must now be attributed to rapid disintegration and interstitial absorption of the tissue.

On the 21st, the fourth injection was made with a solution of 1:2000, and on the 25th, the fifth. The ulcer on this day measured eight and a half by six centim., and was covered with healthy granulations; the insertion of the mastoid muscle showed no infiltration.

After the first injection a small section of the tumor extending through the whole thickness of the growth was excised for purposes of microscopical examination. At this point, the bone had been laid bare, and in the progress of the case became necrotic. Pyæmia was gradually developed, which it was believed (on April 10th, the date of the letter of Prof. Thiersch) would end fatally. Pyæmia was prevalent in the wards of the clinic at the time.

So far, the experiment seems to have established the following points: 1. Remedial agents in solution can be uniformly diffused through soft cellular tissues, in all directions, by subcutaneous injection. 2. If a solution of nitrate of silver of 1:2000 be used, each injection limited to one cubic centimeter, the injection of silver followed after some minutes, or still better immediately, by an injection of a solution of common salt of double strength into the vicinity, and a space of one and a half or two centim. be left between two punctures, an inflammatory reaction, or considerable sloughing, need not be feared in skin, areolar tissue, muscle, or such pathological tissue as was present in this case. 3. It seems doubtless, that rapid disintegration and absorption of morphological elements were the consequences of the action of the remedies; whether this was due to the combination of the nitrate of silver with common salt, or to the former alone, is not decided. Probably similar results may be obtained from other metallic salts.

2—*On Chronic Inflammation of Joints.* By Professor ESMARCH, of Kiel. (*Schmidt's Jahrb.*, CXXX., p. 63.)

In a discourse held before the third meeting of Baltic physicians, the talented surgeon of Kiel (recently deceased) expressed his views on the treatment of those chronic inflammations especially, which we were accustomed to designate as scrophulous arthritis, tumor albus, etc., and exposed the unfortunate results of neglect and the improper therapeutic measures, which so often result from the ignorance, indifference, or prejudices of the people. He charges physicians, also, with too often combating the disease with such unsuccessful heroic remedies, as the hot iron, and the like, leaving the most operative measures untried: "If the prejudices of the people could be removed; if in future a proper treatment of chronic articular inflammations were always employed at the proper time, deaths would seldom occur in consequence of these chronic inflammations, and most patients would be cured completely; *i. e.*, without deformity, and with unimpaired function of the limb."

The two chief remedies which are brought into use in almost all acute and chronic inflammations of joints, with the greatest benefit, are absolute rest of the limb, and the local abstraction of heat. While simple rest in bed operates beneficially upon diseased joints, the effect is even more sure if the good position is rendered immovable by splint apparatus and fracture boxes. But the hardening bandages of plaster of Paris, or starch, have proved the best of all, because by their means an almost absolute rest of limb can be procured; and besides, at the same time, they exert an antiphlogistic influence by uniform compression. They have, besides, the advantage of allowing the patient, in many cases, to leave the bed and enjoy fresh air; many patients, too, can thus be treated *ambulando*. In recent times these hardening bandages have been almost universally found to be extremely practical; but some surgeons have certainly gone too far when they desire almost all species of articular inflammations treated by them. The second powerful remedy by which, particularly in acute cases, the heat, swelling, and pain are reduced, consists in the local abstraction of heat, which is accomplished best, and without the often injurious wetness, by india-rubber bags filled with ice, and placed upon the joint. In all the more acute forms of articular inflammation, the author always first employs cold, which is applied locally until the inflammatory symptoms are moderated. The limb is at the same time properly placed, or still better, fixed by an extension-bandage, which acts beneficially by removing the mutual

pressure of the diseased articular surfaces by muscular tension, and by bringing the joint gradually and gently into a better position. In this permanent extension it is not even always necessary to make counter-extension by a pelvic or perineal belt, the weight of the body often sufficing when the limb is placed in an elevated position.

Concerning the external irritants and derivatives, as salves, plasters, issues, setons, and the actual cautery, the author does not deny their effectiveness in some cases, but thinks that without rest of the limb they certainly are of no use, but may be very injurious; and that their action is inconsiderable as compared with the two chief remedies named.

In articular inflammations, the sooner rest and the local application of cold are used, the more brilliant will be their effect, and complete cures are often accomplished by these means without a trace of remaining functional disturbance. To this end it is indispensable, of course, that the inflammation of the joint is recognized early. If many surgeons still think that the beginnings of a slow articular inflammation often can not be recognized with certainty for some time, the author opposes this view, and is of opinion that an over-sight, or an error in diagnosis, is only possible by improper and defective modes of examination.

3 — *Treatment of Subacute Synovitis, and Contracted Joints.* By Mr. W. ADAMS. (*London Lancet.* April, 1866. p. 257.)

Mr. W. Adams has been employing, for some time past, at the *Great Northern Hospital*, a mode of treatment for effusion into joints, which he finds very satisfactory. In subacute synovitis, especially of the knee-joint, where there is much fluid, a large blister is applied to the skin, and when this has risen, the cuticle is cut away completely, leaving a new surface, which is then dressed with strong mercurial ointment. The dressing is continued so long as the blistered surface remains unhealed. Occasionally, healing takes place very rapidly. In such cases a dressing of equal parts of mercurial and of savin ointment is applied, in order to keep the blister open. No constitutional disturbance or salivation, Mr. Adams tells us, results from this treatment; but he finds the fluid in a joint absorbed with unusual rapidity. The best cases for the employment of this method are those where, after an attack of acute synovitis, although all inflammatory symptoms have subsided, the amount of effusion shows no tendency to diminish. In very chronic cases with much thickening, he considers Scott's dressing more useful. As regards the propriety of employing forcible extension in the contraction which so often follows synovitis, Mr. Adams considers that where this condition ensues upon traumatic or rheumatic affection of the joint, the process is allowable, and may often be employed with great advantage. His plan is to give chloroform, and employ a fair amount of force, not in jerks, but steadily. When the joint is felt to yield, even ever so little, the limb is immediately placed upon a splint provided with power

of extension by screws, which are turned daily. In contraction from strumous disease of joints, he objects even to this amount of violence, having frequently seen grave mischief lighted up by this means. When, however, a joint has been very long quiet, and bony anchylosis has taken place, the strumous taint meanwhile almost disappearing, the objection ceases to apply with the same force, and very careful extension is often found useful.

4—*Treatment of Varicocele by Coagulating Injections.* By Dr. MAISONNEUVE, of Paris. (*Med.-chir. Rundschau*, 1866. I., p. 144; from *Gaz. des Hôpitaux.*)

MAISONNEUVE relates several instances of cure of varicocele by the injection of sesquichloride of iron into the varicose veins of the spermatic cord, a mode of treatment with which he has become familiar for some time past, in varices of the extremities, as one without danger, and offering the best success. He deems this method a great progress, the more so because varicocele has now come to be almost not treated at all on account of the dangers of the operations hitherto in use. The difficulty of applying the method of Pravaz to varicocele consists above all in the uncommon mobility of the veins in the loose connective-tissue, and makes the use of the ordinary troicart, otherwise frequently employed, almost impossible. M. uses the needle of Pravaz' injecting syringe, and a stream of blood immediately informs him that the coats of the vein have been penetrated. The cord of the patient *standing* erect is first compressed upon the pubis by the finger of an assistant, or by a strong truss, and the needle inserted into the most prominent veins, which sometimes succeeds only after several fruitless attempts. If blood escapes, the injecting syringe is applied, and twenty to thirty drops of the solution of iron are injected. The anatomical structure of the venous plexus which ultimately collects into one common trunk, causes the coagulation to take place not alone in the wounded vein, but after a short time in the entire plexus. The pain is said to be inconsiderable, and the inflammation following upon the operation to abate within a few days, so that all the patients mentioned could be declared cured in a short time. Nothing is said of the state of the testicle after the obliteration of its veins.

5—*On the Treatment of Fracture of the Radius at the Styloid Process by means of Gordon's Splint.* By LAWSON TAIT, Esq. (*Braithwaite's Retrospect*, July, 1866; from *Medical Times and Gazette.*)

Few fractures have had so many ingenious splints devised for their treatment as that known as Colles' fracture, yet most or all have been given up, and the ordinary palmar and dorsal straight splints generally used, although frequently with unsatisfactory results.

In spite of the utmost care most cases of this fracture turn out unsatisfactorily, and many are the actions of damages that have been raised on its account. The reason of this non-success is, I think, very plain; let any one examine his own wrist, and the following explanation will be clear. Holding the hand straight out in a plane with the forearm, it will be seen that, while the dorsal aspect is almost a straight line, there is a considerable concavity at the wrist on the palmar aspect; indeed, that a line drawn from the elbow to the ball of the thumb would be, so to speak, the chord of a segment of a circle. Thus it is that when an arm, with the radius broken as it is in Colles' fracture, is pressed by two straight splints, one on either aspect, extending from the elbow to the fingers, the upper fragment must necessarily be pressed towards the palmar aspect of the limb; while the lower fragment, which is practically the same in this condition as the ball of the thumb, is pressed in the opposite direction—in fact, that the distortion is only increased by the splints, as they press the fragments in the very direction in which they are already displaced. If this be correct, then it is easy to understand the success which has attended the use of Dr. Gordon's splint in the treatment of this fracture, and to believe that it is devised on sound anatomical and mechanical principles—that it really is what all splints ought to be, viz., a dermal skeleton.

This instrument was originally invented and described by Dr. Gordon, of Belfast; the only notice, however, which I am aware that it has subsequently received is in a paper by Mr. Stokes in the Dublin Medical Journal. It is composed of two pieces of wood, the one for the palmar aspect of the forearm being about nine inches long, two and a quarter inches wide at the wrist, and three and a half wide at the elbow; the surface to be in contact with the skin is slightly hollowed out to fit the arm, and along its radial border it has screwed to it a wooden bar or pad, which is rounded off at the distal extremity to fit the concavity of the radius; this latter, of course, necessitates that, to fulfill this condition, separate splints are required for the right and left arms.

The pad, in addition to its being rounded off at the extremity, is rounded all along its inner surface so as to press accurately against the radius throughout nearly its whole length, and it is of sufficient height to embrace rather more than half the thickness of the forearm. The other portion of the apparatus consists of a plain piece of three-eight inch board, two inches and a quarter broad, and two inches longer than its fellow; it is for application to the dorsal aspect of the forearm, and has the surface to be in contact with the skin slightly hollowed, and it likewise has its distal extremity transversely rounded. Its application is effected as follows:—The fracture having been reduced, the limb is retained in position by an assistant, the lower part of the apparatus is then applied, padded with spongiopiline or lint, to the radial portion of the forearm alone, and not to the hand. Then the upper splint is to be applied, likewise padded, in such a manner that the proximal ends of the two parts of the apparatus are maintained at the same level, while the distal end of the upper one projects about

two inches beyond the end of the radius. For a more particular description and a drawing, see Dublin Medical Journal, for February, 1865. The whole apparatus is firmly secured by two small straps with buckles. In this manner no pressure is exerted on either of the fragments but what is calculated to keep them in their correct position. The arm, during the after progress of the case, is recommended to be kept in the position most agreeable to the patient, which will be found to be that of almost complete pronation. In the employment of this apparatus the wrist will be found to be confined only to a limited extent, while the movements of the fingers and carpo-metacarpal articulations are quite unimpaired; thus entirely doing away with the most objectionable condition of stiff joints, which is such an annoyance both to surgeon and patient for weeks after the common splints have been removed from the forearm.

6—*On Exarticulation of the Knee Joint.* By Dr. Jos. MAZANOWSKI. (*Archiv f. klin. Chirurgie, VII., p.* 489, —1865.)

The author makes the relation of a case the occasion of a statistical review of the operation, and sums up the advantages, the dangers, and his own views on the best mode of execution, of this operation.

The advantages of the operation still continue to be doubtful, and the difficulties of the after treatment as well as the technical execution of the operation have not been elucidated very precisely, as far as I know, by the most experienced of our masters, and new, but strictly scientific and accurately described observations resting on personal experience, as well as more extensive statistical data on this field may for a long time yet be desirable.

If we study more profoundly the cases which are known to us thus far, it will be seen:

1. The principal advantage of exarticulation of the knee joint, as compared with amputation of the thigh, is this, that after the exarticulation the stump may become useful in walking. In amputations of the thigh this can not be the case, and the patient is compelled to the use of crutches for life.

2. The exarticulation, being performed at a greater distance from the trunk, *possibly* affects the organism less than the amputation. I say possibly, for this does not seem to be statistically proven with certainty. A wound, however, which concerns integuments almost exclusively, i. e. skin, ligaments, and tendons, can not possibly affect the system as much as an incision carried through the entire thickness of the muscles of the thigh and the bone.

3. The surface of the wound after the exarticulation is not as large as after amputation of the thigh.

4. Scarcely any muscles are cut, except the upper ends of the gastrocnemii, and thus all muscles indispensable to the motion of the thigh are left intact.

5. The stump is covered with skin, able to bear the pressure in walking upon an artificial leg, and a large surface for support is obtained.

6. A very important advantage of the exarticulation is, finally, that the femur need not be sawed through. Every experienced surgeon well knows what consequences a femoral amputation sometimes has. During the late Crimean war I have not unfrequently seen them. The femur may become necrotic in the stump, and form a tubular sequestrum several inches long; the healing process may then require years, if the patient does not in the mean time succumb to the exhaustion, or various dangerous complications: pyaemia, pseudo-erysipelas, abscesses, etc.

I shall regard the exarticulation in the knee as *indicated*, wherever the amputation of the thigh can be avoided by it, i. e., 1. Where the condyles of the femur are healthy; 2. Where the skin is so far uninjured that it will yield a sufficient flap.

It is *contraindicated*, 1. When the condyles of the femur are in a diseased condition; 2. Where the skin around the knee joint, by reason of wounds, abscesses, fistulæ, etc., is diseased and altered, so that it is impossible to gain a good covering for the stump.

Regarding the dangers during the after-treatment of the exarticulation, we must guard against the following accidents:

1. Most dangerous in exarticulation of the knee joint seems to be *the tendency to secondary hæmorrhages* in the stump. In my case it was repeated *four times*. The same took place in a case known to me, operated upon by Prof. Maisonneuve, which ended fatally. The tendency to secondary hæmorrhages must be still greater, when the operation is performed according to the method of Baudens, in which the popliteal artery is cut at a point where it sends off numerous branches. This circumstance unavoidably impedes the formation of a thrombus in the tied vessel, the more so because the wound is disturbed by

2. The *retraction* of the flexors of the thigh and the cut portions of the gastrocnemii, which withdraw upwards and backwards, while their antagonists, the extensors, retract upwards and forwards, with the patella gliding upon the smooth surface of the inferior femoral epiphysis as upon a roll. For this reason I regard first intention as almost impossible; the wound is opened and strained, and suppuration and formation of abscess ensues in the whole large surface of the synovial capsule as far as the upper sinus and the bursa of the extensors.

3. The retraction of the muscles also causes the protruding of the cut end of the sciatic nerve, which occurred in my case also, and probably caused the enormous pain in the case of Prof. Vanzetti, quoted above. * * * * * Concerning the execution of the operation, I believe myself justified in proposing the following:

1. The best method for the exarticulation of the knee appears to me to be the circular incision, in which opinion my most highly honored teacher, Prof. von Langenbeck, * * * * * concurred. In this method the popliteal artery is cut further from its divisions, consequently less danger from secondary hæmorrhages is threatened. Markoe and Pick operated by a large anterior and a small posterior flap, which sometimes indeed may be indispensable. An oval incision may sometimes also be used.

2. The branches of the sciatic nerve protruding from the stump must be excised.

3. As far as possible, it must be sought to obtain healing by first intention in the posterior part of the wound, by carefully applied sutures.

4. It seems to me quite purposeless to remove the patella, provided it is completely sound. Nay, it is far better always to preserve it so as to lengthen the stump, even if it should unite with the condyles of the femur. But if a part of its mobility could be preserved, the patella would be very useful to the patient in walking. This, however, is not so difficult to accomplish, if the above-mentioned retraction of the muscles is taken into consideration. It is known to every experienced surgeon, that the contraction of the flexors of the thigh is greater than that of the extensors, and that the cicatrix, in very successful femoral amputations healed by first intention, is always directed far upwards and backwards. About the same takes place in disarticulation of the knee. To take advantage of this, we must subcutaneously cut the ligament of the extensors transversely, from the inner surface of the wound. Thus, I believe, the wound is enabled to heal by first intention; the patella then can not glide upwards and in front. But when the attempt at first intention does not succeed, it would be very appropriate to make a transverse or semilunar opening with the convexity directed upwards, by means of a transverse incision through the ligament of the extensors. This would enable us to separate the patella entirely from the extensors during the whole period of healing. Such an aperture might also be very useful as a counter-opening, for the discharge of the pus and of the secretions of the *bursa extensorum*, as a preventative of abscess in the superior sinus of the synovial capsule. It is now well known, that not only a subcutaneous, but even a percutaneous tenotomy of the *tendo Achillis* is by no means so dangerous as was formerly thought. The displacement of the patella upwards by the retraction of the extensors, seems to me the main cause of failure of Gritti's operation, the unfortunate imitation of the excellent operation of Pirogoff. Here, also, this tenotomy of the extensors, I believe, would yield more favorable results.

5. After the exarticulation, *the femoral artery must be permanently compressed by graduated compresses*, accurately applied in the lower part of its course, and by some descending turns of the roller, or by a plaster-of-Paris bandage of the stump, in order (*a*) to diminish the pressure of the blood upon the ligatures; (*b*) to prevent burrowing of pus upwards, and (*c*) to secure the absolute rest of the muscles and stump, indispensable to the healing process.

6. The after treatment of the exarticulation must be more careful than that of other operations. Abscesses, and suppurating cavities, which form more frequently under the ligament of the extensors, and in the upper sinus of the synovial capsule, must be laid open early, and by long incisions; the more so, if the above-mentioned transverse incision and counter-opening had not been made in the beginning. The bandage must also be well managed to maintain the patella as much as possible in a fixed position at the apex of the condyles.

An exarticulation of the knee, with attention to all the observations above related (and punctually to be fulfilled), might, I am convinced, yield far more favorable results than those at present known.

OPHTHALMOLOGY.

1—*On the Necessity of Employing Greater Accuracy in Ascertaining and Expressing the Degree of Acuteness of Vision.* By H. DERBY, M. D., of Boston. (*New York Medical Journal, July*, 1866.)

This is a subject of absorbing importance. It is the text in which ophthalmology records its triumphs. That it should be clear, defined, terse, and accurate, none will deny, as upon its possession of these qualities depends the value of our scientific observations.

The older ophthalmologists gave the results of their treatment, operative or medicinal, in a vague and unsatisfactory manner. We are informed that the vision in one case remained imperfect; in another, became so much improved that the patient could read ordinary type, or even fine print. One could see objects about him nearly as well as ever; another received imperfect impressions. This information is very well as far as it goes, giving, as it does, an approximate idea of the benefit resulting from the operation or treatment. But in view of the vast advances recently made in the single department of operative ophthalmic surgery, it will be readily admitted that far greater accuracy of expression is required in order to decide on the relative merits of rival operations, to ascertain exactly how far the degree of vision, retained or acquired, falls short of, or how it compares with, the normal standard. The subject to be discussed during the present session is an instance in point. Of what value would be the statistics of a hundred cases of extraction of cataract where it was simply stated that the patient afterwards read newspaper print or great primer type, no allusion being made to the rapidity or confidence with which this was accomplished, to the distance at which the paper or book was held, or to the perception of distant objects? I have, for example, at present, a patient laboring under an amblyopia brought about by the inordinate use of tobacco. With one eye he reads Jaeger No. 6 (brevier); with the other Jaeger No. 4 (nonpareil). This sounds like a fair amount of sight. And

yet this very patient, put to the tests now in common use, is discovered to have lost in one eye eleven-twelfths, in the other thirteen-fourteenths of his vision. And the principle long received, that a large retinal image aids the eye to overcome numerous circles of dispersion, enables us to account for the size of type made out when held in close proximity.

It is, therefore, no longer sufficient to say that the eye makes out a certain size of ordinary type, and it is for such reasons that the useful system of Jaeger (so great an advance on what had preceded it) has yielded to that which we owe to the genius of Snellen. With his formula we are all familiar. "The utmost distance at which the types are recognized, divided by the distance at which they appear at an angle of five minutes, gives the acuteness of vision." $V=\frac{d}{D}$ certainly deserves to come next in order to the formula of Donders for expressing the range of accommodation, the stepping stone to his labors in that field.

I think, however, that no one can long watch the progress of his own cases and record results on the plan of Snellen, without being sensible of certain deficiencies in its present application. And I am desirous of very briefly calling your attention to a leading source of inaccuracy, viz., *the varying degree of illumination.*

That the amount of vision is dependent on the intensity of light has been recognized by Snellen, and in his edition of 1866 he states that the illumination of the letters should be of constant value during our experiments. The majority of us have doubtless been struck with the varying answers obtained from the same person when examined at different times of the day, and in different states of the weather. Fractions of different value are obtained in the same case within so brief a space of time as to preclude the possibility of any appreciable change in the vision having taken place. I do not think it necessary to dwell on this fact, entertaining no doubt that the experience of all who use the test of Snellen by ordinary daylight will bear me out in this respect.

On the principle, then, that "the illumination of the letters should be of constant value during our experiments," I would suggest an expedient which must have often occurred to every one present, and which is now brought forward with the conviction that its general adoption is a matter of considerable consequence. *Let the letters used as a test be placed in a room from which daylight is excluded, and let them be illuminated by a steady flame, uniform in intensity of illuminating power, and placed always at the same distance from the letters.*

I have followed out this plan for the past four years with great advantage, and find that its adoption not only insures accuracy in the examination, but gives confidence to the patient. Nothing is more common than to find that patients who have been already examined elsewhere, and who bring with them the results of that examination, endeavor to account for any apparent variation on the plea of the light being different, or the time of day not being the same. Those, however, who come month after month, and year

after year, and whose vision is tested on the same or similar letters, hanging in the same darkened room, and illuminated constantly by the same light, are easily convinced that any change that may have occurred is in the eye, and not the surroundings.

Each practitioner can readily decide by experiment what degree of illumination will render the letters of the text visible in the proper distance to the average emmetropic eye. And the general adoption of this principle is strongly urged, with the conviction that in this way alone can we obtain results at once reliable and uniform.

2—*On Black Cataract.* By HAYNES WALTON, Esq. (*Braithwaite's Retrospect*, July, 1866; from *Brit. Med. Journal.*)

[This form of cataract is almost unknown to the profession, except by a paper read before the Royal Medical and Chirurgical Society a few years ago, by Mr. Walton. By a naked eye examination no cause of blindness can be discovered; but by the ophthalmoscope it is seen that the lens is of a very deep color. Whenever the cataract has been extracted in any of the few cases which have been met with, it has always been of a deep brown color.]

An actual black cataract has been lately removed by Mr. Walton, of which we shall give the particulars, as there are some points of physiological interest connected with it.

A man, 65 years of age, was sent to Mr. Walton by Mr. Philbrick, of Colchester, because of his blindness. In both eyes there was the congenital defect, coloboma iridis, or deficiency of a segment of the iris, in a marked degree. His sight was so little affected by it that he worked successfully as a gardener, and his employers never knew that he had any defect.

The coloboma is sometimes accompanied with a split or fissure in the retina. It is impossible to say whether this existed; probably not, because of the quality of the sight.

Except that there were a few minute dots of gray in the pupil of the left eye, nothing could be recognized there, as all else was black. Even when the light of the ophthalmoscope was concentrated on it, nothing more could be seen; but as the interior of the eye could not be illuminated, sufficient light not entering for the purpose, it was evident that there was some black body that intercepted the rays There was scarcely any sight in the eye, only the mere perception of light from darkness.

Mr. Walton operated by extraction, and made the lower section of the cornea, because the iris was cleft in that direction. The cataract proved to be quite black, and of the usual consistence. It was put aside for a minute examination, but unfortunately was destroyed. The gray markings spoken of must have been in the capsule, for no such appeared on the cataract.

The case proceeded without a bad symptom, and the cornea healed well, but there was no beneficial result. There is no more sight than existed before the operation. This important question then arises, Are those

changes in the interior of the eye, by which the eye is spoiled, and the nature of which can not be ascertained by the ophthalmoscope when the crystalline lens is lost, associated with the peculiar alteration of the lens? Do they go together as allied diseased states, or is their association accidental?

But a few years ago, every case of loss of sight, in which the pupil looked black, was called amaurosis. It is very likely that many a black cataract was so designated. This shows how necessary it is to use the ophthalmoscope, both for the positive and the negative information we get from it.

In the right eye also there was a cataract. The center of it was quite black, while a small marginal portion was rather deep brown than black. The interior of the eye could not be illuminated by the ophthalmoscope. There was very little sight. Mr. Walton did not operate; he thought it most probable that the back of the eye was defective, as on the opposite side; and the patient was not at all willing for an operation.

3—*On Amblyopia and Amaurosis, caused by the Abuse of Tobacco and of Alcohol.* By Prof. SICHEL. (*Med.-chir. Rundschau*, 1865, No. 43; from *Annales d' Oculistique.*)

Among the cerebral amauroses there are two kinds, both frequent and difficult to cure; namely, the amaurosis by abuse of tobacco, and that by excessive use of spirituous liquors. The former was first pointed out by MACKENZIE, and SICHEL is led, after an experience of twenty-eight years, to confirm it. The amblyopia from either of these causes is characterized by the absence of any intense or even demonstrable cerebral congestion; the ophthalmoscopic signs are either negative or at least of little prominence; sometimes the memory is impaired; both sometimes occur in the same individual.

The treatment is difficult. When signs of cerebral congestion are present (which is rare), antiphlogosis and revulsion by intestines and skin is indicated; when they are absent, bloodletting is injurious; mild purgatives, sometimes alternating with more active ones, are of use. When the affections are of a purely passive character, stimulants are required; irritating ointments to the temples, flying blisters, the internal use of camphor, arnica, strychnine, etc. The first condition of cure, of course, is the abstinence from tobacco, *resp.* alcohol.

METEOROLOGY OF JUNE, JULY, AND AUGUST, 1866.

BY GEORGE ENGELMANN, M. D., ST. LOUIS.

	JUNE.		JULY.		AUGUST.	
	1866.	Average.	1866.	Average.	1866.	Average.
Barometrical Pressure—						
Mean	29″.465	29″.477	29″.517	29″.513	29″.511	29″.526
Highest	29″.827	29″.965	29″.704	29″.867	29″.739	29″.940
date	29th.	4th, 1859.	12th.	6th, 1859.	5th & 16th.	3d, 1842.
Lowest	29″.091	28″.990	29″.360	29″.195	29″.128	29″.128
date	4th.	2d, 1842.	3d.	18th, 1862.	31st.	31st, 1866.
Temperature—						
Mean	74°.3	74°.4	81°.7	79°.0	74°.5	76°.5
Highest	94°.0	99°.5	98°.0	104°.0	100°.5	101°.5
date	25th.	9th, 1836.	27th.	21st, 1860.	12th.	4th, 1861.
Lowest	50°.0	48°.0	62°.0	58°.0	48°.5	44°.5
date	18th.	5th, 1839.	1st.	4th, 1859.	24th.	30th, 1863.
Evaporation	8°.0	7°.9	7°.9	8°.6	8°.2	7°.7
Relative Humidity	64.2	65.3	68.3	65.1	64.5	67.5
Rain—						
Quantity	5″.59	5″.66	3″.67	4″.17	5″.16	4″.15
Days on which it fell	12	10	9	7	6	7
Thunder-storms	13	9	8	6	4	5
Principal Winds	SE.	SE.	SE.	SE.	NE.	SE.
Cloudiness	3.7	3.9	4.2	3.6	3.1	3.6
Fair days	8	11	10	14	14	14
Variable days	22	17	19	17	17	16
Days without sunshine	0	2	2	1	0	1
Stage of River—						
Mean, feet	17′.8	18′.6	16′.4	17′.9	12′.7	12′.5
Highest	20′.5	41′.4	17′.3	39′.3	14′.6	26′.7
Lowest	15′.2	8′.8	14′.9	6′.6	11′.3	3′.8

COMPARATIVE METEOROLOGY OF THE SUMMERS AT ST. LOUIS.

	1866.	1865.	Average.
Barometrical Pressure—			
Mean	29″.498	29″.533	29″.505
Highest	29″.827	29″.765	29″.965
date	June 29th.	July 22.	June 4, 1859.
Lowest	29″.091	29″.161	28″.990
date	June 4th.	July 19.	June 2, 1842.
Temperature—			
Mean	76°.8	76°.3	76°.6
Highest	100°.5	97°.0	104°.0
date	Aug. 12.	July 5.	July 21, 1860.
Lowest	48°.5	55°.0	48°.0
date	Aug. 24.	June 26, Aug. 23.	June 5. 1839.
Evaporation	8°.0	7°.1	8°.1
Relative humidity	65.7	70.9	66.0
Rain—			
Quantity	14″.42	15″.11	13″.98
Days on which it fell	27	27	24
Thunder-storms	25	21	20
Principal Winds	SE., NW., NE.	SE., next SW.	SE.
Cloudiness	3.7	4.2	3.7
Fair days	32	37	38
Variable days	58	51	50
Days without sunshine	2	4	4
Stage of the River—			
Mean, feet	15′.6	18′.1	16′.2
Highest	20′.5	26′.8	41′.4
Lowest	11′.3	11′.3	3′.8

The above tables give the meteorology, the first that of the three summer months, and the second that of the summer as a unit compared with the same periods of last year and with the average of thirty years. The investigation of the condition of the atmosphere during the last months is of greater than usual interest, because within that time the great epidemic of this century has again made its appearance among us, making it the duty of the meteorologist to investigate any possible connection of the atmospheric conditions with the spreading of the cholera, or any influence the former may have exercised on the latter. I am aware that this question is not a new one; but I know as well, that very different solutions have been given; some more specious than others, but none to me quite convincing. I do not expect to solve the difficulties; I only propose to add to the stock of knowledge by giving the results of careful observations.

The tables show that June was every way an average month. The atmospheric pressure was a little below the mean, but the temperature and the quantity of rain were almost equal to the average; the number of fair days a little less, and the number of rainy ones and of thunder-storms a little more than usual. The range of temperature was much lower than we often observe it; for it was neither as warm nor as cool as it frequently is in June.

July was also an average month as regards atmospheric pressure and quantity of rain, but the temperature was 2.7 degrees higher than the mean temperature of the month; and this was owing, not so much to unusually elevated degrees of heat, as to constant high temperature from the fourth to the end of the month, during which period the thermometer rarely fell below 70, and almost daily rose to 90, though seldom over 95 degs., while a temperature of 98 to 100 is not rare with us in July. The quantity of rain was a little below the average, while the number of days on which it rained, the cloudiness, the relative humidity, and the number of thunder-storms was greater. Heat and humidity combined began to tell on the sanitary condition of the inhabitants, and bilious diarrhœas, and dysentery, together with remittent and intermittent fevers, made their appearance among our, until then, unusually healthy population; sporadic cases of cholera morbus began to show themselves, not, however, any more than we see every summer under similar circumstances.

While July was warmer, August proved to be much cooler than this month usually is in our climate. Its mean temperature was fully two degrees below the average, while the highest temperature of the summer also occurred in this month. The atmospheric pressure was a little below the average; the humidity and cloudiness were much less, and the evaporation greater than is usual in August; the quantity of rain was also greater. This seeming anomaly, however, is easily explained if we take into consideration that by far the largest quantity of rain (three and a quarter inches) fell on the evening of the last day of the month, which fall of rain rather belongs to the month of September, initiating the uncommonly wet season of that month. Otherwise, August had

only five rains, of which only one (on the 12th) was of any importance. After July 21st there was in fact no rain (with the exception of a slight sprinkle on August 7th) until August 13th, while the July heat continued almost without intermission till then, and culminated on the day before (August 12th), reaching a point above 100 degrees. The second half of the month was characterized by dry, clear, and unusually cool weather, with a prevalence of northeasterly winds. During the first week of the month the same class of diseases prevailed which were noticed in July. Cases of cholera began to multiply, until after the first week of the month it manifested itself as an epidemic, which increased in extent and in intensity until the last week of August, when it began to abate.

The summer, as a whole, was an average one in atmospheric pressure, temperature, evaporation, relative humidity, and quantity of rain; we had more thunder-storms than usual (as many as last year), and fewer fair days; the winds were more variable, and not so predominantly from the southeast as is common in our summers.

As I have said above, the weather during the greater part of July was warm and moist, with a prevalence of easterly and southeasterly winds. July 16th to the 21st we had daily rains, mostly accompanied by thunder-storms, and the heaviest on the 19th. After the 21st the weather became more settled, clear, and dry, and quite hot, and continued so for three weeks until August 12; at first with northeasterly, afterwards with northeasterly and southeasterly winds prevailing. From July 21st to August 1st the temperature never fell below 70 degrees at sunrise, nor below 90 in the warmest part of the day. In the following eleven days of August the atmosphere cooled off somewhat, so that on the morning of the 5th the thermometer indicated only 60 degrees, while on that day and on the 9th it scarcely reached 88 degrees in the afternoon. The heat soon rose again, until on the 12th, the warmest day of the summer, it exceeded one hundred degrees; a violent thunder-storm and rain followed in the evening of that day. A period of delightful,

clear, cool, and bracing weather succeeded, until the last day of the month, during which only two very light showers scarcely moistened the surface of the parched soil. The thermometer ranged in the morning mostly between 50 and 60 degrees, and in the early afternoon between 70 and 80; only in the three first and the four last days of this period it rose above eighty, and solely on the 31st over 90 degrees. With the evening of August 31st a spell of wet weather set in, quite unprecedented in this season in our climate, which lasted until September 25th. During those twenty-six days, only eight were without measurable rain, though on some of them a dark and misty atmosphere made it worse than rain.

During all this time, embracing almost three months, the barometer showed no marked variations, but was, as is usual in this season with us, remarkably quiet and steady. From July to the middle of August it was rather high; from the middle to the end of August somewhat lower than usual, and during September higher again, as is commonly the case in that month.

We can, then, distinguish four well marked periods in these three months: (1) A warm and moist period from July 4th to the 21st. (2) A hot and dry period from July 22d to August 11th, succeeded by two rainy days, the 12th and 13th, which formed the transition to (3) the period of cool, clear, and dry weather, from August 14th to the 30th; and, (4) a cool and very wet period, from August 31st to September 25th.

Now let us see how cholera appeared during these four periods: During the first period, of warm and moist, I might almost say tropical weather, so favorable to the generation of such diseases, we had only isolated cases of sporadic cholera morbus; during the second period, of hot and dry weather, it gradually but slowly increased, and assumed the epidemic form; in the third period, of cool, clear, and dry weather, healthy, bracing, and recuperative as it ought to have been, the epidemic raged the worst, but abated suddenly towards the end of the month; during the fourth period, of cool and wet weather, it at first increased again, but then rapidly decreased, and approached, it is believed, its termination.

What, then, had the atmospheric influence to do with the advent, the spread, and the cessation of the epidemic?

Evidently very little; their effects were only secondary in so far as wet and cool weather predisposed to bowel complaints, and thus, it is believed, the sudden change of August 31st, and the succeeding cool weather, may have induced the recrudescence of the epidemic just at that time. But we must admit that the delightful weather of the middle and latter part of August can in no way be connected with the rapid increase of the cholera during most of its continuance.

The meteorological observations, therefore, give only negative results in regard to the march of this epidemic, and atmospheric conditions, evidently, have only a minor influence on it.

Experience has, however, proven, that in winter the cholera has always disappeared; or, at least, has become greatly lessened. It is a disease of warm weather, which comes to us from warmer climates.

The cholera epidemics of 1849, 1850, 1851, 1852, 1854, and 1855, all agree in the great regularity of their course. They began or increased in April, augmented rapidly in May, reached their acme in June or July, and decreased still more rapidly in August, and generally died out in September or October. I thought I had discovered in this six times repeated regular course a stable law, according to which the epidemic increased with the, to it, most congenial condition of our atmosphere in the fore part of summer—the wet, sultry, hot weather of June and part of July, when we see cases of sporadic cholera in every year, and very abruptly left us with the advent of hot and dry weather, under the prevalence of atmospheric conditions which produce typical forms of disease—bilious remittents and intermittents.

I am not now convinced that I was wrong in that position, though our present epidemic commenced when those of the years mentioned above abated. The experience of this year only proves that cholera *may* also appear late in the summer and fall. It does not invalidate the theory that the foresummer is with us the season most congenial to its full and fatal development. It

may be added, that it was in the month of August, in the year 1832, that cholera for the first time appeared in St. Louis in an epidemic form; that it ceased during winter, and prevailed again in the following summer of 1833. I recollect having seen several violent cases in June of that year. If it does at all revisit our city next season, it is almost certain that June will be the month when it will be most prevalent and most pernicious.

I refrain from following up the subject of cholera and the laws of its propagation any further, because foreign to the subject of meteorology, but must be permitted to say one more word about the drinking water that is used in our city. Contrary to an officially-promulgated opinion, which condemns our river or hydrant water, and recommends the use of cistern and well water, it is my own and most other physicians' experience, that it is much safer to make use of our river water than to drink well water. I have no experience about cistern water, but presume that in the city it to a great extent shares the bad qualities of well water, though it lacks its hardness. Both kinds of water are liable to be impregnated with the organic impurities accumulating in a large city from a thousand sources, even besides gutters, privies, and sewers, which percolate through the soil, especially where it is not composed of the solid diluvial clay of our hills, but of the porous soil of our bottom lands forming the lower part of our city, or, still worse, of the indiscriminate fillings up of many former ravines, valleys, or sinkholes. Our river water, distributed through our water works, impure as it is, is certainly healthier than either well or cistern water. It is extensively used in almost every family, and in thousands of families, among whom no case of cholera has occurred; while we have known in this visitation, as we did in 1849, many wells in different parts of the city, around which cholera cases, as it were, clustered—for a great part, at least, undoubtedly induced by the use of their water.

As our well water will get worse with the increase of the number and the density of our population, a pure and healthy hydrant water becomes every year a more urgent desideratum, and such water can only be furnished us by our great Mississippi.

ATMOSPHERIC ELECTRICITY, TEMPERATURE, AND HUMIDITY.

Observations made at St. Louis, Missouri, based on daily observations at 6, 9, 12, 3, 6, and 9 o'clock, from morning till night.

BY A. WISLIZENUS, M. D., ST. LOUIS.

1.—*Monthly Mean of Positive Atmospheric Electricity.*

JUNE.

YEAR.	6 A. M.	9 A. M.	12 M.	3 P. M.	6 P. M.	9 P. M.	MEAN OF MONTH	NO. OF THUNDER STORMS.	PREVAILING WINDS.
1861...	3.8	3.6	4.1	4.6	4.8	3.0	4.0	7	SE.
1862...	2.9	3.2	3.7	2.7	3.1	2.5	3.0	6	S.,SW.,NW.,N.,SE.
1863...	3.4	1.9	1.8	1.3	2.2	1.3	2.0	6	NW., SE. & SW.
1864...	3.3	4.0	5.6	5.5	3.8	2.0	4.0	2	NE. & SE.
1865...	2.7	3.1	2.8	3.9	6.0	1.9	3.4	4	S. & SW.
1866...	3.2	1.7	1.1	1.8	2.2	2.8	2.1	16	S. & SE.
Mean.	3.2	2.9	3.2	3.3	3.7	2.2	3.1	7.0	.....

JULY.

1861...	3.7	3.0	4.0	4.0	4.5	3.3	3.7	3	S.
1862...	1.9	1.5	1.8	2.0	3.1	2.7	2.2	10	SE.
1863...	3.2	1.5	3.8	2.9	3.6	2.1	2.8	5	SW. & SE.
1864...	2.5	1.9	2.2	2.0	2.3	2.9	2.3	3	SE.
1865...	2.2	2.3	2.5	3.3	3.2	2.0	2.6	11	SW. & S.
1866...	2.5	1.2	2.2	1.8	4.2	2.4	2.4	9	S. & SW.
Mean.	2.7	1.7	2.7	2.7	3.5	2.6	2.7	7.0	.....

AUGUST.

1861...	2.1	3.0	3.7	4.8	4.4	2.3	3.4	5	NE.
1862...	1.7	1.6	3.2	2.5	3.9	1.2	2.3	7	SE. & NE.
1863...	4.3	4.4	4.5	4.2	6.7	2.4	4.4	12	SE. & S.
1864...	1.0	0.6	0.7	0.9	1.3	0.9	0.9	4	SE.
1865...	5.6	5.0	6.3	7.8	7.2	3.5	5.9	6	NE. & SE.
1866...	7.9	6 1	3.7	4.1	4.7	4.4	5.1	3	NE. & SE.
Mean.	3.8	3.4	3.7	4.0	4.7	2.4	3.7	6.2	.....

2.—*Monthly Mean of Temperature, Fahrenheit.*

JUNE.

YEAR.	6 A. M.	9 A. M.	12 M.	3 P. M.	6 P. M.	9 P. M.	MEAN OF MONTH.
1861	68.5	76.6	81.5	82.7	79.8	72.5	76.9
1862	66.5	74.9	79.7	81.5	77.1	71.2	75.1
1863	61.8	72.1	76.8	78.3	75.0	67.6	71.9
1864	68.8	79.1	84.0	86.2	81.6	73.9	78.9
1865	72.7	81.0	85.6	86.6	82.6	75.0	80.7
1866	65.6	77.8	80.0	81.6	76.8	70.0	75.8
Mean	67.3	76.9	81.3	82.8	78.8	71.7	76.5

JULY.

1861	67.9	77.5	82.8	83.9	80.1	73.0	77.5
1862	72.8	81.6	86.1	87.3	83.0	76.7	81.2
1863	67.9	77.1	82.8	83.9	79.9	71.4	77.2
1864	74.3	84.1	88.4	90.1	85.4	78.6	83.5
1865	70.5	78.8	82.0	81.9	79.7	73.6	77.7
1866	73.7	82.9	86.6	87.8	84.2	77.9	82.2
Mean	71.2	80.3	84.8	85.8	82.0	75.2	79.9

AUGUST.

1861	67.8	78.8	83.8	84.7	81.5	75.0	78.6
1862	71.8	81.0	85.8	86.9	82.3	76.2	80.7
1863	69.0	76.9	83.3	84.1	79.2	72.5	77.5
1864	70.3	78.0	83.8	85.2	80.5	74.9	78.8
1865	71.0	77.3	82.3	84.1	80.0	74.1	78.1
1866	66.0	75.9	82.9	84.2	79.2	72.6	76.8
Mean	69.3	78.0	83.6	84.8	80.4	74.2	78.4

3.—*Monthly Mean of Relative Humidity.*

JUNE.

YEAR	6 A. M.	9 A. M.	12 M.	3 P. M.	6 P. M.	9 P. M.	MEAN OF MONTH.
1861	86.2	70.5	60.6	59.4	66.4	81.9	70.8
1862	82.2	68.1	58.6	55.6	63.5	79.1	67.0
1863	85.1	64.5	56.7	56.2	62.8	80.9	67.7
1864	76.5	59.6	50.2	49.6	58.2	75.1	61.5
1865	81.9	65.3	58.2	56.4	63.8	82.0	67.9
1866	82.6	60.4	57.2	54.0	63.1	79.0	66.0
Mean	82.4	63.9	56.9	55.2	63.0	79.7	66.8

JULY.

1861	84.3	64.0	56.4	54.8	60.9	77.4	66.3
1862	82.7	64.1	56.5	54.3	62.8	80.7	66.8
1863	87.4	67.8	57.7	55.7	63.7	79.4	68.6
1864	79.1	61.7	53.1	51.1	59.3	72.2	62.8
1865	85.1	71.4	66.2	67.2	72.2	84.5	77.4
1866	85.7	65.0	56.9	57.2	65.3	79.4	68.2
Mean	84.0	65.7	57.8	56.9	64.0	78.9	68.3

AUGUST.

1861	91.9	68.9	59.2	56.4	62.8	78.5	69.6
1862	88.0	63.0	52.4	48.7	60.6	78.4	64.3
1863	87.3	71.6	59.5	55.8	67.6	82.2	70.7
1864	85.3	69.2	59.1	57.3	65.5	77.4	69.0
1865	88.5	71.7	63.9	60.0	69.1	82.3	71.7
1866	85.8	65.6	55.2	54.3	62.1	77.0	66.7
Mean	86.1	68.3	58.2	55.4	64.6	79.3	68.7

REMARKS.—In the foregoing meteorological tables the month of August is of peculiar interest, because the long dreaded epidemic of cholera invaded our city during this month, and carried off several thousand victims. The epidemic, which for months had been stationary in the East, made about the end of July a sudden jump towards the West, and appeared here earlier than we expected it, and without the usual premonitory symptoms.

It is so natural to connect this most mysterious of all diseases with atmospheric influences, that from its first beginning, scientific men have tried to discover the cause of its origin and propagation in that direction; but, thus far, all meteorological observations have proved nothing definite. If changes in atmospheric pressure, temperature, or humidity seemed sometimes to occur in causal connection with the disease, at other times they were entirely wanting; and the epidemic, defying meteorology, appeared in fair and unfair weather; in warm and cold seasons; in rain and in sunshine. Intense cold only, and high elevation above the sea seemed to check its advance.

Let us compare now the meteorology of August, 1866, with the same of other years, to see if any material difference can be found with which the epidemic might be connected.

The atmospheric pressure, as appears from the tables of Dr. Engelmann, offers nothing peculiar; the barometer was at about its usual height and range.

The month was generally dry, the quantity of rain somewhat above the mean, but the greater half of it fell on the last day of the month and in the night of the 1st day of September.

The prevailing wind was, as in August of last year, the north-east wind.

Both the temperature and the relative humidity of the month were several degrees below their average mean; but that has often happened before without an epidemic of cholera. The temperature was rather unequally divided throughout the month. The first half of August was quite hot, and the second decidedly cool: on the 12th of August, my thermometer reached 101.5, and on the 24th, it fell as low as 49.5. The greatest mortality occurred in the cooler part, from the 17th to the 24th of August. This change in temperature may have exerted some influence upon the number of cholera cases, but is of course not the cause of the previously existing epidemic.

Another meteorological phenomenon has often been suspected and directly connected with cholera epidemics; to-wit, Atmospheric electricity. Being itself a mysterious force,—being less carefully observed and studied, atmospheric electricity is the bug-bear to which all strange diseases are generally referred. I am not aware that during former epidemics of cholera, either here or in Europe, regular reliable observations of atmospheric electricity have been made with exact instruments. Nevertheless, in many medical journals it has been pronounced as a fact, that during epidemics of cholera atmospheric electricity was almost wanting, and that this want of electricity was therefore the real cause of the disease. Occasional observations, made at random, and the greater or less facility in working electrical machines, form generally the foundation for such a statement. Having made for the last six years regular daily observations of atmospheric electricity with Dellman's instrument, and having continued them during the present epidemic, I am enabled now to speak with more certainty upon this subject.

The mean quantity of positive atmospheric electricity in the month of August, 1866, was 5.1, while the average mean of August within the last six years is but 3.7. Instead of a decline we find, therefore, an increase in atmospheric electricity. Does that warrant us to assume that the larger quantity of positive atmospheric electricity in this month was the cause of the epidemic? By no means. In looking over the tables we perceive that the mean quantity of atmospheric electricity in August, 1865, was still larger, to-wit, 5.9. In both months the same winds were also prevailing, northeast and southeast. But there was no cholera at all in August, 1865, and an awful epidemic in August, 1866.

The number of thunder-storms in August, 1866, was but three, while the average number is six. But there raged in the night, from the 12th to the 13th of August, such a protracted and violent thunder-storm, with such a profusion of lightning, that it might well be considered equal to four thunder-storms. The two weeks following that thunder-storm exhibited the highest mortality during the epidemic, just as in 1849 the greatest mortality of the epidemic appeared after "the great fire," proving how little the epidemic is influenced in its course by local purifiers of the atmosphere, even of such intensity as thunder-storms and large fires.

I have often tried in vain to make observations of ozone in St. Louis. Like in other large cities, all the ozone of the atmosphere is here absorbed by sulphurous, ammoniacal, and other exhalations, with which it readily combines. But a friend of mine, Mr. Ad. F. Bandelier, of Highland, Madison county, Ill., has for several years made there very careful observations of ozone, and has favored me with the following comparative result of ozone observations in August, 1865, and 1866:

The mean monthly quantity of ozone in August, 1865.....4.29
The mean monthly quantity of ozone in August, 1866.....3.82
The average mean of ozone in June, July, and August, 1865.....4.15
The average mean of ozone in June, July, and August, 1866.....3.70

"The less quantity of ozone in the summer months of 1866 was not accidental, but constant throughout the observations.

The daily variation of ozone, the difference in day and night observation, was in the summer months of 1866 much less than in those of 1865. August, 1865, showed the greatest daily difference of 2.2, while August, 1866, gave the smallest one of 0.6."

Ozone and its coefficient antozone are formed, according to the latest researches (especially by Meissner), by polarization of the common neutral oxygen; ozone representing the negative, antozone the positive pole of oxygen. But, as the subject is by no means yet exhausted, we are not warranted, on account of a slight difference in the quantity of ozone, to express an opinion in regard to the connection or disconnection of ozone with cholera. Mr. Bandelier mentions, also, that up to the present date no case of cholera has appeared in Highland.

Having reviewed thus all the meteorological phenomena of August, I can not reduce any one singly, or their aggregate, into a direct connection with the prevailing epidemic. During the greater part of the month, and just when the disease was most fatal, the weather was as fair and moderate as in Indian summer days, so that this discrepancy between the weather and the epidemic created general astonishment. I am therefore driven to the conclusion, that our meteorological observations give but a negative result as to their direct connection with the epidemic, and that the so-called weather exerts but a limited and indirect influence upon it; not more than any excesses or sudden changes in temperature, relative humidity, electricity, etc., will generally exert upon the health of a community.

If meteorological phenomena are excluded as direct causes of cholera epidemics, in what other direction shall we look for them?

The effects of cholera upon the system are so similar to those of acute poisoning, that the word cholera-poison has been adopted by physicians of all countries. What constitutes, now, the supposed, but never elaborated or demonstrated cholera-poison? Is it a poison proper, acting by chemical agency? or is it one of those lowest infusorial organisms, cryptogamic or animalcular, propagating themselves most rapidly under favorable circum-

stances by cells or spores, like ferment? The infusorial theory would account most readily for the mysterious origin and spreading of cholera epidemics; for their sudden disappearance and absence for many years; for their propagation by cholera discharges, etc. Like higher organisms, insects for instance, such infusoria, if they should prove to be animalcular, may undergo regular metamorphoses, and their germs may, by favorable or unfavorable external influences, sometimes be destroyed and sometimes be preserved by millions, as it happens with the locust, the grasshopper, the army worm, etc. But, unfortunately, this is for the present a mere theory, because no ocular proof of such an infusorial organism has as yet been given by the mieroscope. But microscopy is only at the beginning of its wonderful career, and we may still entertain the hope that, at some future day, it will solve the riddle of the origin of cholera.* In which element, then, should microscopical researches for that purpose be instituted, in the air or in the water? Ehrenberg has found that the air is nearly as full of infusorial life as water; but since attacks of cholera begin always in the digestive organs and not in the lungs, it is more probable too that food or drink carry the poison into our system, and that water, the most general drink, is in all likelihood the most general vehicle for its introduction.

This corresponds, also, with the result of Pettenkofer's laborious researches, according to which localities with stagnant, underground water are especially infected with cholera. If this theory, that water forms the principal vehicle for the development of cholera-poison, should prove correct, it might explain, too, why St. Louis, which from its situation ought to be one of the healthiest cities, fares so badly during cholera epidemics. Both in 1849, and in the present epidemic, St. Louis has suffered comparatively more from cholera than any city in the Union. But, alas! we are also cursed with the vilest and filthiest drinking-

* A great step forward in a similar direction has lately been made by Dr. Salisbury, who, after long and careful experiments, found the cause of our intermittent and remittent fevers, the so-called malaria, in the minute spores of palmellæ (a kind of algæ), which, in night time, are elevated with the moist air to a height of thirty and more feet. See American Journal of Medicine, etc., January, 1866.

water of any city in the country. The Mississippi water itself is not very inviting, on account of the great mass of sediment it contains, and only becomes palatable when settled and filtered. But imagine this water now collected in a large reservoir, in which this mud has been carefully preserved for years, so that the bottom of the reservoir, as of late was officially stated, holds a firm deposit of twenty-seven feet in thickness! What a glorious field for exploration this rich bottom-land must offer to the geologist, the palæontologist, the zoologist, the botanist, the chemist, the microscopist, and the toxicologist!

From Oken's Urschleim (primitive slime), from which organic life sprang in the beginning, down to the latest sweet-water formation, every branch of those sciences might collect there abundance of interesting material for their cabinets; while the humble citizen, who pays for his share of the supernatant liquid, is only once in a while so fortunate as to discover in his obstructed hydrant the remnants of an asphyxiated fish, or a live lizard or salamander. But these gross, heterogeneous substances which fall under the observation of all our senses, though disgusting, are after all not so injurious as the more subtle atoms of decomposed organic matter which escape our senses, but gradually affect our system by fermentative processes, producing diarrhœa, or keeping up at least a disposition for it. In ordinary times this tendency to diarrhœa may be controlled by diet, stimulants, and medicines, and the system may at last become used to it; but in an epidemic of cholera, when diarrhœa constitutes the first stage of the disease, the influence of such water must be doubly injurious. If cholera-poison is not directly developed in it, such water must certainly increase the liability to choleraic attacks.

In various parts of the city well water is sometimes used, but as the wells are generally dug in alluvial ground, and in narrow yards, are surrounded by privies, gutters, and sewers, a subterraneous connection often takes place between them, and the water of such wells, impregnated with atoms of organic ferments, becomes even more injurious than our turbid hydrant water. In 1849, and during this epidemic, I have observed some such wells

forming the center of an infected neighborhood. If our citizens wish, therefore, to protect St. Louis in the future from such pernicious epidemics, let them first of all provide for an abundant supply of clear, healthy water, free from inorganic substances, and free from organic poisons.

In corroboration of what I have stated, I insert here a chemical analysis of our drinking water, made lately by an expert chemist of our city, Dr. Theodor Weiss, at the request of the Board of Water Commissioners. His paper on the subject, instead of being hidden in the archives of the Board, deserves wider circulation, and with the kind permission of its author, I lay the general result of his researches before the scientific readers of this Journal:

1.—*Result of Chemical Analysis of water taken from the Reservoir of St. Louis, on July* 14, 1866, *in quantities of two pounds, and carefully filtered.*

The residue collected on the filter contained:	1—Water taken from the surface of Reservoir near the inlet.	2—Water f'm the surface of Reservoir near the outlet.	3—Water f'm the surface in the centre of Reservoir.	4—Water below the surface close to the inlet to the hydrant pipes.
a.—Organic Substance	0.75 grs.	0.65 grs.	0.85 grs.	3.66 grs.
b.—Silica	0.35 "	0.35 "	0.55 "	26.33 "
c.—Alumina	1.95 "	1.75 "	2.05 "	4.75 "
d.—Oxyd of Iron	0.08 "	0.08 "	1.00 "	3.00 "
e.—Carbonate of Lime	0.75 "	0.65 "	1.00 "	2.50 "
f.—Carbonate of Magnesia	0.40 "	0.35 "	0.45 "	1.30 "
Total	4.28 grs.	3.83 grs.	5.90 grs.	41.54 grs.

The filtered waters gave all the same result, to-wit, in two pounds:

a.—Albumen	1.45 grains.
b.—Sulphate of Lime	1.65 "
c.—Sulphate of Alumina	0.65 "
d.—Sulphate of Magnesia	0.80 "
e.—Oxyd of Iron	0.01 "
f.—Muriate of Magnesia	0.45 "
Total of substances in solution	5.01 "

2.—*Result of Chemical Analysis of water taken from different localities on August* 1, 1866.

The residue on the filter contained:	1—Water f'm the middle of river near the New Bremen ferry.	2—Water f'm the inlet to the Reservoir.	3—Water f'm the outlet of the Reservoir.	4—Water f'm the hydrant in Locust street.
a.—Organic Substance	0.40 grs.	0.42 grs.	0.51 grs.	9.20 grs.
b.—Silica	0.45 "	0.45 "	0.45 "	10.30 "
c.—Alumina	1.65 "	1.65 "	1.65 "	17.40 "
d.—Protoxyd of Iron	0.02 "	0.02 "	0.02 "	1.45 "
e.—Carbonate of Lime	0.60 "	0.60 "	0.60 "	15.70 "
f.—Carbonate of Magnesia	0.30 "	0.30 "	0.30 "	8.85 "
Total	3.42 grs.	3.44 grs.	3.53 grs.	62.90 grs.

Chemical Analysis of the filtered waters gave the following result:

a.—Alumina	1.35 grs.	1.45 grs.	1.55 grs.	1.70 grs.
b.—Sulphate of Lime	1.70 "	1.71 "	1.72 "	1.70 "
c.—Sulphate of Magnesia	0.85 "	0.86 "	0.85 "	0.85 "
d.—Sulphate of Alumina	0.71 "	0.72 "	0.71 "	0.71 "
e.—Protoxyd of Iron	0.01 "	0.01 "	0.01 "	0.01 "
f.—Muriate of Magnesia	trace.	trace.	trace.	trace.
Total	4.62 grs.	4.75 grs.	4.84 grs.	4 97 grs.

Dr. WEISS made also a preliminary examination of the water of the Merrimac, and found it much clearer and purer, with only a trace of albumen (organic substance), but the quantity was too small for a quantitative analysis.

Having gone, as practising physicians, through both the cholera campaigns of 1849 and 1866 in St. Louis, I will add but a word concerning the treatment of the disease. If our theoretical knowledge of its character has thus far not much advanced, we can neither boast much of great progress in its treatment. All new remedies heralded as specifics, as for instance, the concentrated solution of camphor, the permanganate of potassa, the hyposulphite of soda, the nitrate of bismuth, the ice-bag to the spine, etc., have all proved failures. The hypodermic method has been found sometimes useful when the stomach is too irritable, and the internal use of chloroform affords often a valuable adjuvant of camphor. But, generally speaking, the physician had to fall back, as in the epidemics of 1832 and 1849, upon the judicious use, at the different stages of the disease, of our four heroic medicines, opium, calomel, camphor, and quinine.

A. W.

Editorial Department.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

A Treatise on the Origin, Nature, Prevention, and Treatment of Asiatic Cholera. By John C. Peters, M. D. New York: D. Van Nostrand. 1866. 12mo., pp. 162.

Cholera: Facts and Conclusions as to its Nature, Prevention, and Treatment. By Henry Hartshorne, A. M., M. D., etc., etc. Philadelphia: J. B. Lippincott & Co., 1866. 16mo., pp. 79.

Cholera: its Characteristics, History, Treatment, etc., etc. By William B. Fletcher, M. D. (Reprinted from the *Cincinnati Journal of Medicine.*) Cincinnati, 1866. 8vo., pp. 57.

Epidemic Cholera: its Pathology and Treatment. By A. B. Palmer, M. D., Professor, etc., etc. (Reprinted from the *Detroit Review of Medicine and Pharmacy.*) Detroit, 1866. 8vo., pp. 33.

The visit of epidemic cholera has been preceded and accompanied by the appearance of quite a number of treatises and essays upon cholera; all of these which we have seen confine themselves to the limits of an octavo pamphlet or a moderate duodecimo, and are rather deficient in classic worth. The work first on our list, is one of the best, for its material has been collected with diligence and care. Dr. Peters relates *much* that has been said of cholera, but with very little critical judgment, and lays "claim to no great originality, except for the development of the Physiological Theory, and the advocacy of the internal disinfectant and corrective treatment."

The author believes thoroughly in the infectious nature of Asiatic cholera: it is "both portable and communicable," and "the infectiousness belongs peculiarly, if not exclusively, to the matters which the cholera patient discharges by vomiting and purging." Much space is devoted to the discussion of the origin and mode of distribution of the disease, and the theory of its portability by "persons, ships, clothing, and baggage," by

water, and dust, etc., is fully carried out. But the symptomatology and pathological anatomy of the disease — the corner-stones of its pathology — are utterly neglected, and we regard this as the chief defect of the book.

The author next gives us a summary of seven different theories as to the nature of true cholera, without indicating at all which of these meets his approbation, except that, in the preface, he claims originality for the "Physiological Theory." This latter consists in the idea, that the secretion of the immense amount of serum in the intestinal canal is normal, its accumulation resulting from the almost absolute cessation of absorption, which is caused by the partial or entire removal of the intestinal epithelium: not over-secretion, therefore, but want of absorption. Yet he is not quite true to even this theory, for he states (p. 83) that—

"As the intestinal glands are greatly enlarged, they probably pour out a much greater quantity of fluid than natural;"

which is the doctrine of the "Gland Theory" of Dr. Isaac Hayes; of this, our author remarks, that it is "*almost as satisfactory* as the physiological one. . . . " Why he should choose the latter euphonious epithet for his own doctrine in contrast with the rest, is not clear. And if Dr. Peters heartily believes in his own theory of non-absorption, the following sentence is remarkably inconsistent:

"The indications of treatment according to this ("*physiological*") view, are to use *tonic astringents*, especially iron, alum, sulphate of iron, simple alum, gallic acid, muriate (*sic*) tincture of iron, elixir vitriol, sulphuric acid, etc."

According to our notion, the theory would call for measures favoring absorption, not for astringents.

Throughout the book we find a certain loose way of accepting theories, and some statements of very doubtful assumptions as positive facts, of which the following will give an example:

"As *ozone* is deficient in the air in cholera times, steps should have been taken to supply it; for ozone is a vital element of the air, and oxygen can not be assimilated by the system without its presence." (p. 108.)

How unfortunate for the dwellers in cities; for ozone observations show, that there is often no ozone at all in the atmosphere of large cities, and always very little as compared with the open country. The theory that oxygen is converted into ozone during respiration, more especially by the hæmatine of the blood, is likely; but that the oxygen of the atmosphere without the presence of ozone should be unfit for assimilation, we have never heard asserted before.

The chapter in which the sentence last quoted occurs—on the prevention of cholera, and the following one, on the management of patients and nurses—are the most commendable in the treatise. In the former, the first section advocates the most thorough system of quarantine, a position which we can not share with the author; the sections on cleanliness and on disinfection are admirable and very complete. The author's management of the sick and of the nurses, though very correct and efficient in our opinion, presupposes wealth and no limit to the number of attendants; it is practicable only in upper-tendom.

The long exposé of treatment is a failure. Dr. Peters gives all possible therapeutical methods with very little attempt at discrimination; indeed, it is very difficult to find out his own method of treatment. For "prodromic diarrhœa" he recommends thirty-four prescriptions; for "choleraic diarrhœa" twenty-seven, "from which any intelligent physician can easily make a choice." In the treatment of fully developed cholera this desire for completeness leads our author into some sorry contradictions. He tells us that "little or no absorption takes place, and the largest and smallest doses of the most active and mildest medicines are alike powerless;" nevertheless he recommends a liniment containing "tinct. nux (*sic*) vom.," and a page further on, "Seltzer, Vichy, or carbonic acid water, with the addition of a little *iron*, or some iron water, like Pyrmont," for drink. *Cui bono?*—The treatment of collapse, finally, consists in letting the patient recover or die, whichever he chooses.

Then follows a kind of Materia Medica of cholera, which is very complete, and therefore useful to any one desirous of studying the resources of the therapeutical apparatus in this dire disease.

If we have severely criticised the treatise before us (from which certainly no one can learn to *diagnose* a case of cholera), and have found many and serious defects, yet let the length of our notice bear testimony that we have not found it devoid of worth and useful information.

It remains to pay our tribute to the publisher, who has left us nothing to criticize in the *extérieur* of the book. Its clear type and general neatness make us express a warm desire to see Mr. Van Nostrand engage more extensively in medical publication.

Dr. HARTSHORNE'S little treatise is in direct contrast to the former; it shows throughout but one decided, fully "made-up" opinion, and is as free from vacillation and doubt as the subject will allow. We are pleased to note, also, that we have here found an author with whom we can heartily agree in his opinions on the nature and causation of cholera.

Cholera is "an acute systemic affection," whose "morbid cause acts through the blood" upon the nervous system; it is "a poison-spasm; a ganglionic tetanus." The cause is not cosmic or telluric, but material; it is not a gas, for "the diffusion of all gases through space prevents the local accumulation of gases everywhere, unless constantly emanating from a terrestrial source." But we know of no *migratory* source of emanation. Cholera is not contagious, but infectious; and the cause is organic; nay, probably organized. Quarantine is not available; "it never has succeeded, and never can," in practice, even if sound in theory; "its infraction for smuggling and other inducements is everywhere constant and notorious; this can not be prevented; one might think the history of blockade-running, during the late rebellion in this country, might afford ample illustration and confirmation of this." "I insist that SANITARY POLICE includes the sum total of available measures for the prevention of cholera in any place." For personal prevention,

"one principle will suffice here: *to keep the system at par;* neither above its level of excitement, nor below that of its due strength." Dr. H.'s treatment is "by antispasmodics and mild stimulants, in small doses at short intervals; with ice, and external frictions, etc.;" the remedies used are, in the main, chloroform, camphor, and opium. "Let us confess honestly, for it is wise to do so, our art is here very weak!"

Our limited space prevents more than a glance at the other pamphlets whose titles appear above. Dr. Palmer's essay, couched in admirable language, is diffuse rather than complete; it is devoted chiefly to treatment: opium, camphor, and calomel, are the main resources; and the author calls especial attention to the frequent complication of cholera with malarial influences, and the necessity of administering quinine in many cases. Dr. Fletcher's pamplet, which we see small reason to compliment, is accompanied by a very useful and suggestive map, "showing the range of cholera epidemics,"—but also by very inferior microscopic drawings, the originality of which is not apparent from the text. (Fig. 2 and 3 are from Kirchenmeister and from Bennett,—copies of bad artistic execution.)

G. B.

The following books and pamphlets have been received:

SHAKSPEARE'S DELINEATIONS OF INSANITY, IMBECILITY AND SUICIDE. By A. O. KELLOGG, M. D., Assistant Physician State Lunatic Asylum, Utica, New York.

This is a small, but very neat book, of 204 pages, divided into three parts:

PART I.—*Insane:* Lear; Macbeth; Lady Macbeth; Hamlet; Ophelia; Jaquess; Cordelia.

PART II.—*Imbeciles:* Bolton; Dogberry; Elboio; Shallow; Malvolio; Bardolph; Nym; Pistol; Launce; Caliban.

PART III.—*Suicides:* Othello.

These essays were published in the Journal of Insanity at intervals between 1859 and 1864, and are now shaped into a nice

little book that is worthy of perusal by every physician. For sale by publishers, Hurd & Houghton, No. 459 Broome street, New York. 1866.

AN ADDRESS TO THE MEMBERS OF THE MISSOURI DENTAL ASSOCIATION. By the President, H. E. PEEBLES, D. D. S. St. Louis, 1866.

We have read this essay upon the duties and interests of the dental profession with no small appreciation of the manner in which the author has addressed his brethren. We are pleased to observe the interest shown in Missouri by this department of surgical science.

The subjects of discussion for the next Annual Meeting, are: I. Mechanical Dentistry; II. Sensitive Dentine; III. Anæsthetics; IV. Alveolar Abscess; V. Irregular Teeth; VI. Action of Acids on the Teeth; VII. Treatment of Superior Second Molar, with a Posterior Approximal Cavity Extending to the Cavitas Pulpæ; VIII. Dental Ethics; IX. Treatment of Six Year Molars; X. Miscellanea.

MEDICAL ELECTRICITY; embracing Electro-Physiology and Electricity as a Therapeutic, with special reference to Practical Medicine. Showing the most approved Apparatus, Methods, and Rules for the Medical Uses of Electricity in the Treatment of Nervous Diseases. By ALFRED C. GARRETT, M. D. Third edition. Revised and Illustrated. Philadelphia: J. B. Lippincott & Co. 1866. For sale by Keith & Woods, Fifth street, St. Louis, Mo.

This is a beautifully bound book of about 1100 pages, in elegant type and paper. The work appears a worthy one. We will in a future number examine it more carefully, and give our readers a brief view of its contents.

VALEDICTORY ADDRESS OF THE NEW ORLEANS SCHOOL OF MEDICINE, SESSION 1865-6. By J. L. CRAWCOUR, M. D., Professor of Medical Chemistry and Legal Medicine. New Orleans, 1866.

We have read with pleasure this worthy address. It is small, but full of noble thought, and excellent advice to the graduates of the New Orleans College, by one of our most distinguished professors.

THE PROSPECTUS OF THE KANSAS CITY MEDICAL AND SURGICAL PIONEER.

A monthly journal, published at Kansas City, by our old friend, J. Keller, M. D. We wish him and the enterprise much success.

MEDICAL DIAGNOSIS; with special reference to Practical Medicine. A Guide to the Knowledge and Discrimination of Diseases. By J. M. DA COSTA, M. D. Engraving on Wood. Second edition. Revised. Philadelphia: J. B. Lippincott & Co. 1866. For sale by Keith & Woods, St. Louis.

This is a new and much improved edition of Da Costa's excellent work that we have previously noticed. It should be in the possession of every physician.

THE PRIMARY SURGERY OF SHERMAN'S CAMPAIGNS. By E. ANDREWS, M. D., Professor of Surgery in the Chicago Medical College, and J. M. WOODWORTH, M. D., Chicago, 1866.

A NEW PLASTIC OPERATION FOR CERTAIN DEFORMITIES OF THE FACE. By E. ANDREWS, M. D., Professor of Surgery in the Chicago Medical College. Chicago, 1866.

These are both excellent essays, clear, concise, and bold—in the manly style of our old friend, the author. They should be sent for by every surgeon.

ON SPERMATORRHŒA: ITS CAUSES, SYMTOMATOLOGY, PATHOLOGY, PROGNOSIS, DIAGNOSIS, AND TREATMENT. By ROBERTS BARTHOLOW, A. M., M. D., Professor of Physics and Medical Chemistry in the Medical College of Ohio; Lecturer on Clinical Medicine and Physician to St. John's Hospital, Cincinnati, etc. New York: Wm. Wood & Co., 61 Walker street, 1866. For sale by Peter Smith, 184 North Sixth street, St. Louis, Missouri.

REFLEX PARALYSIS: ITS PATHOLOGICAL ANATOMY AND RELATION TO THE SYMPATHETIC NERVOUS SYSTEM. By M. GONZALEZ ECHEVERRIA, M. D. (University of Paris), Physician to the Charity Hospital, New York, etc., etc. Balliére Brothers, 520 Broadway, New York. 1866.

INSTRUCTIONS IN THE PREPARATION, ADMINISTRATION, AND PROPERTIES OF NITROUS OXIDE, OR LAUGHING GAS. By GEORGE T. BARKER, D. D. S., Professor of Principles of Dental Surgery and Therapeutics in the Pennsylvania College of Dental Surgery, etc., etc. Philadelphia: Burlingame & Stockton. 1866.

EIGHTH ANNUAL COMMENCEMENT OF THE CHICAGO MEDICAL COLLEGE, LAKE FOREST UNIVERSITY. Chicago, Illinois. Session 1866–7.

TWENTY-FIRST ANNUAL COMMENCEMENT OF THE OHIO COLLEGE OF DENTAL SURGERY, College street, Cincinnati, Ohio, 1866.

CATALOGUE OF THE OFFICERS AND STUDENTS OF THE ST. LOUIS UNIVERSITY, Missouri, 1865–6.

MISCELLANY.

CHOLERA PREVENTIVE.

A few words on this subject will, I hope, be considered *apropos* just at this time; let us first state a few admitted facts:

1. Of the cause of cholera we so far know nothing; but we know that it is a something of the nature of an infection, which, introduced into the blood and acting in it as a ferment, causes it to become stationary in the veins,—a sort of necræmia of the blood,—which is followed by the separation of the liquid from the solid parts of that element, constituting a true serous hemorrhage through the stomach and bowels.

2. Whatever may be the theory as to the cause of this so-called zymosis or fermentation (and either Liebig's albuminoid, or Pasteur's animalcular, or any other theory may be adopted), it must be admitted that there are two morbid elements in the blood in cholera, as in other zymotic diseases; the one is of the nature of a ferment or excitor of change, and the other is the material capable of fermentation. When either of these is absent from the blood, zymosis is impossible; but when both are present, then such a reaction takes place that zymotic phenomena ensue. Now, the ferment may be rendered innocuous, although it may not be readily destructible itself, by destroying the fermentable material, or at least by changing its composition, that fermentation is effectually hindered.

3. We also know that every fermentation is an oxydation, and that by removing the oxygen we stop that fermentation.

4. We know that in the presence of sulphurous acid gas no fermentation can take place, on account of the great affinity of that substance for oxygen, with which it forms sulphates, etc. Hence, guided by experience, the wine makers of France and Germany, before filling with wine their new casks, burn in the casks through the bung hole a piece of cloth saturated with sulphur, to produce in the cask sulphurous acid gas, which uniting with the oxygen in the pores of the wood, combines with it to form sulphuric acid, etc., which is washed out, then the

wine put in, without any fear of its going through a further degree of oxydation (acetic fermentation), by which it would turn into vinegar. The cider makers of this country, with the same view, use the sulphite of lime, etc.

5. The above views, first presented to the public by Dr. G. Polli, of Milan, have been repeatedly reproduced in several late numbers of *Braithwaite's Retrospect*, and it is to be regretted that the value of the researches of that able physiologist have not been duly appreciated in this country; but the time is near when the application of these new facts will be considered a boon for humanity, equal, if not superior, to the discovery of vaccination.

Dr. POLLI, in his search after a safe anti-zymotic agent, has found it in the sulphites and hypersulphites of soda, potassa, and magnesia. These sulphites have been found to act, not upon the vital processes, but upon the fermentable something, without which the ferment is innocuous, and in this way they constitute safe and efficacious anti-zymotics. Among the facts of Dr. Polli's researches, we shall briefly state, for instance, that he selected a number of dogs, a proportion of which he dosed with the sulphites, while the remainder were left without. He then killed them all, the weather being warm. He found that the blood, the urine, the viscera, and the flesh of the sulphited dogs resisted putrefaction for many days, while the same elements of the unsulphited animals were already putrid. The obvious conclusion from so striking a fact is, that as the sulphites prevented putrefactive fermentation in the dead body, they would, *a fortiori*, prevent it in the living. He selected two or three dogs, as nearly equal in size and state of health as possible, and inoculated them with the nasal mucus of a glandered horse. One of the dogs had had previously administered to it from four to five scruples or more of the sulphites of soda or of magnesia daily for some days, and then was treated by sulphites as soon as inoculated; a third was left without treatment, either prophylactic or curative. The result was that the third animal died, and the others recovered. He injected, with like results, fetid pus in the veins of dogs;

experimented on oxen, etc., the flesh of the sulphited oxen remaining fresh several weeks, while that of the non-sulphited animals, killed the same summer day, became putrid in four or five days, etc.

This valuable suggestion soon became popular in Italy, and has been applied successfully in England in many cases of zymotic diseases, as a prophylactic and curative agent. The beneficial effects of the sulphites have been clearly demonstrated in measles, scarlet fever, puerperal fevers, pyæmia, septicæmia, and all classes of fever with putridity and purulent absorption.

In view of the above facts, and admitting cholera to be of a zymotic or infectious nature, I have by analogy, early during this epidemic, resorted to the use of the sulphites and hyposulphite of soda as a preventive of the disease. I have prescribed it extensively among families where fatal cases of cholera had already taken place, and in the most infected districts of this city, to upwards of a hundred persons much exposed to the infection, and no instance, so far, have I seen a case of cholera among those who used the remedy.

The sulphite of soda, on account of its alkalinity, answers also another very important purpose not claimed by Dr. Polli; it preserves the blood in that state of liquidity which is in itself the best preventive of cholera; as the best cure of cholera are the remedies which restore the fluid condition of the blood, such as the liquid ammonia, the salts of soda, or potassa, and also calomel. I use the liquid ammonia (aromatic spt. ammoniæ), not the carbonate, because my object is to make carbonate of ammonia out of the excessive carbon of the blood, thus purifying it. And it is a well established fact, *en passant*, that men who work in stables, or occupations where ammonia is evolved, never or seldom take the cholera, because they inhale continually that ammonia, which keeps their blood in a *super*-fluid condition.

I will be forgiven, I hope, these hasty remarks on account of the great importance of the subject, and I will give the formula which I have, after several trials, adopted. I use the sulphite in preference to the hypersulphite of soda, because the latter is apt

to generate very disagreeable and offensive flatulency; and I use also in this formula, a small proportion of tincture of catechu to obviate the tendency the remedy has to slightly loosen the bowels. Each tablespoonful contains twenty-four grains of the salt. This may be safely persevered in several weeks or for a month; its use should then be interrupted for a week or so, on account of the great deoxydising quality of the drug; the use of which for so long a time might be followed by œdema, or diseases of debility. I must say that the above dose is not the maximum; as much as a drachm of the salt has been given internally and safely for several days:

Sodæ sulphite,	℥iss
Aquæ anisii,	℥iv
Syrup aurantii,	℥vi
Tinct. cinchon. comp.,	℥ij
Tinct. cardamom. comp.,	℥ii
Tinct. catechu,	℥i

Dose.—One tablespoonful three times a day before meals; half a dose for a child twelve years old. I invite all my friends and others to turn their attention as soon as possible to this subject, and to give a fair trial to the sulphites in cholera and other zymotic diseases. These views may prove useful, if not this, perhaps next year, as we will in all probability be visited again by the epidemic. L. Ch. B.

We have heard Prof. Linton recommend "Don Quixote," and, if our memory serve us right, "Tristram Shandy," as medical text books and companions to the practitioner, who should be able to treat his patients—*pro re nata*—to a dose of Attic salt. An idea of the same sort seems to have actuated our worthy Board of Health in relieving their many and onerous duties during the epidemic by the publication of the humorous notice we have been reading in our daily papers. The grave face put upon the recommendation of such prophylactics against cholera, as a teaspoonful of sulphur in each sock, or the inevitable piece of camphor in one's pocket, which is officially made known to the

public over the signature of the health officer, "by order of the Board of Health," is well calculated to save the public mind from the depressing influence of our daily melancholy experience. But the honorable Board has omitted the recommendation of a mode of treatment which we are convinced would do even more to elevate the spirits of the public. In the March number of the London *Lancet* (New York edition), on page 175, Mr. JOHN GASON, F. R. C. P. Ireland, with no less gravity, prevents the collapse of his patients by stopping the rice-water discharges by means of a "roller" of towel so applied as to effectually plug the anus. *Probatum est,—at least Mr. Gason tried it!*

G. B.

ANNOUNCEMENT OF THE MISSOURI DENTAL COLLEGE.

SESSION OF 1866-67.

This institution was chartered by the State of Missouri during the present month (September, 1866), and is now completely organized by the appointment of a full Board of Trustees, Corporators, and a Faculty of eight professors, and will go into operation on the first Monday in October. We are more than pleased to recognize the Dental Associations of the various states, the General Association of the United States, and the several Dental Colleges, as essential parts of the great brotherhood of Doctors. We are gratified to know, that the dentists of Missouri are alive to the importance of a thorough collegiate course of instruction, in preparing students for the practice of dentistry.

Having previously declared by resolutions in the State Association that no dentist shall turn out a student to practice without a diploma, they have now established a regularly incorporated college in St. Louis, to be known as the MISSOURI DENTAL COLLEGE.

The first course of lectures will be delivered in the halls of the St. Louis Medical College, and commence on the first Monday of October, proximo. We are looking for a full class. May great success attend the enterprise. For further information see the

announcement in the advertising columns of this Journal, or by addressing

HOMER JUDD, M. D.,
Dean of Faculty,
Office, Northeast cor. 6th and Pine Sts.

NOTICE.—We are again unfortunately under the necessity of apologizing for the lateness of the present number of the Journal, which has been unavoidable in consequence of the severe epidemic of cholera that has prevailed among us during the last two months, causing a delay in the preparation of the Scientific Reports and Mortuary Statistics. We trust the fullness and value of these interesting and important parts of the Journal will compensate our readers for the delay.

ALSO, much valuable matter in the department of Book Reviews and Editorials, already in type, has been crowded out of this number, but will appear in the December issue; among which are the Surgeon General's Circular, No. 6, and Radcliff on Paralysis, and much other important matter.

☞ The ensuing volume, for 1867, will be so enlarged as to prevent the recurrence of this difficulty in the future. Please refer to the announcement of the Journal. The present number, it will be seen, is sixteen pages larger than usual, on account of the press of matter, and may be considered a *specimen* of each number for 1867.

☞ Our patrons are respectfully referred to the announcement in the advertising columns, which, we trust, will be carefully considered. It will be observed that certain important changes as to size, form, and price, are proposed if sustained by our subscribers. During the year we have inserted two lithograph photographs, one of Prof. Pope, and the other of Dr. John Barnes, with brief biographies, *at a large expense* and labor, without solicitation. This we wish to continue if our friends will sustain us, commencing with a picture and biography of Prof. M. L. Linton, M. D.

Will our friends favor us with their names at once, and transmit their subscription *on reception of the first number?* If a prompt return is made, we will be able to make the change, and issue a sufficient number to meet the demand at the commencement of the year. *Show us your interest in the* WESTERN SERIAL!

OBITUARY.

At a meeting of the St. Louis Medical Society, held Sept. 22, 1866, the following preamble and resolutions were adopted:

WHEREAS, The epidemic which has prevailed in this city has taken from us one of our recent members, Dr. NATHAN HAYWARD, while in the active pursuit of his profession; therefore,

Resolved, That the Society, in view of the character and training of the deceased, and the just expectations of his future, recognizes in Dr. Hayward's death a serious loss to the medical profession in St. Louis.

Resolved, That we hold no death to be more honorable for the physician, than to fall while contending with pestilence.

Resolved, That the Secretary be instructed to send a copy of these resolutions to the family of the deceased, as a mark of our respect and sympathy, and that they be published in the St. Louis Medical Journal.

WILLIAM JOHNSON, M. D., *President.*

R. H. ANDERSON, M. D., *Recording Secretary.*

Mortuary Statistics for the Month of June, 1866.

Apoplexy, 4; Atrophia of Liver, 1; Abscess, 1; Accident, 1; Burn, 2; Bronchitis, 5; Convulsions, 39: Cerebral Congestion, 1; Congestive Fever, 2; Cholera Morbus, 6; Cerebro-Spinal Arachnitis, 1; Cerebro-Spinal Meningitis, 1; Cancer of Uterus, 1; Congestion of Bowels, 2; Cirrhosis of the Liver, 1; Cerebritis, 5; Croup, Membranous, 5; Congestive Fever, 1; Congestive Chills, 2; Cholera Infantum, 20; Cholera, 1; Compression of Brain, 1; Congestion of Lungs, 3; Congestion of Brain, 10; Dysentery, 11; Diarrhœa, 27; Debility, 23; Diphtheria, 6; Dentition, 1; Disease of Heart, 2; Drowned, 4; Dropsy, 7; Erysipelas, 6; Encephalitis, 1; Enteritis, 5; Eclampsia, 3; Endocarditis, 1; Effusion on Brain, 1; Frost Bite, 1; Gastro-Enteritis, 1; Gastritis, acute, 1; Hydrops, 2; Hydrocephalus, 7; Hæmoptesis, 1; Hepatitis, 1; Hepatitis, chronic, 1; Hyperæmia of Lung, and Bowels, 1; Influenza, 1; Inflammation of Bowels, 3; Internal Hemorrhage, 1; Inanition, 2; Jaundice, 2; Lock Jaw, 1; Marasmus, 18; Moribund, 1; Melancholia, 1 Meningitis, 14; Nervous Fever, 1; Œdema Pulmonum, 1; Phthisis, 37; Premature Birth, 4; Pneumonia, 19; Puerperal Fever, 1; Puerperal Mania, 1; Peritonitis, 2; Phrenitis, 1; Paralysis of Lungs, 1; Remittent Fevers, 5; Rupture of Uterus, 1; Rubeola, 1; Still Born, 26; Scurvy, 1; Suicide, 1; Sun Stroke, 1; Scrofula, 1; Scarlet Fever, 3; Tabes Mesenterica, 2; Tubercular Cachexia, 2; Typhoid Fever, 7; Trismus, 5; Typhus Fever, 1; Typhoid Dysentery, 1; Ulceration of Bowels, 2; Unknown, 4; Variola, 7; Wounds, 2; Wounds of Head, 8. Total, 416.

Nativity: United States, 313; England, 7; France, 1; Ireland, 34; Germany, 53; Italy, 2; Prussia, 3; Scotland, 1; Holland, 1; Canada, 1. Total, 416.

Mortuary Statistics for the Month of July, 1866.

Apoplexy, 6; Angina Membrana, 1; Asthenia, 1; Abscess, 2; Ascites, 4; Atrophy, 1; Bronchitis, 2; Congestion of Brain, 88: Cholera Infantum, 54; Convulsions, 72; Congestion of Lungs, 4; Cerebritis, 6; Cramp, 2; Cholera, 14; Collapsus Nervos, 1; Contusion, 2; Cerebral Congestion, 1; Congestive Chills, 5; Concussion of Brain, 2; Carotitis, 6; Congestive Fever, 9; Catarrh, 4; Cyanosis, 1; Colitis, 3; Cancer, 3; Congestion of Stomach, 1; Carcinoma, 2; Cholera Morbus, 17; Debility, 34; Dysentery, acute, 22; Dysentery, chronic, 2; Diarrhœa, acute, 57; Diarrhœa, chronic,

12; Drowned, 9; Dentition (dif.), 4; Diphtheria, 2; Dropsy, 2; Enteritis, 9; Erysipelas, 1; Eclampsia, 1; Epilepsy, 1; Encephalitis, 5; Gangrene, 3; Fever, 5; Hydrocephalus, 18; Fall, 5; Heart Disease, 6; Hemorrhage, 1; Hydropsy, 1; Hepatitis, 8; Inflammation of Uterus, 1; Inflammation of Brain, 1; Intermittent Fever, 1; Inanition, 1; Inflammation of Bowels, 2; Lock Jaw, 1; Lightning, 1; Marasmus, 21; Moribund, 5; Meningitis, 21; Myelitis, 1; Malformation, 1; Mania a Potu, 5; Pneumonia, 16; Phthisis, 41; Puerperal Fever, 2; Pertussis, 7; Paralysis, 2; Parturition 1; Premature Birth, 4; Poison, opii., 1; Poison, arsenic, 1; Polydipsia, 2; Peritonitis, 3; Remittent Fever, 5; Retention of Urine, 1; Rubeola, 4; Still Born, 32; Scarlet Fever, 2; Scrofula, 2; Suicide, 2; Sun Stroke, 10; Scorbutus, 1; Typhoid Fever, 25; Trismus, 6; Typhus Fever, 1; Tetanus, 2; Tabes Dorsalis, 1; Tuberculosis, 1; Unknown, 18; Ulceration of Bowels, 11; Variola, 1; Wounds, 5. Total, 725.

Nativity: United States, 344; France, 2; England, 10; Ireland, 70; Germany, 87; Italy, 4; Prussia, 4; Switzerland, 2; Greece, 1; Canada, 1. Total, 725.

Mortuary Statistics for the Month of August, 1866.

Abscess, 1; Atrophy, 6; Asthenia, 56; Apoplexia, 3; Bronchitis 2; Burn, 4; Colica Pectonum, 1; Congestive Chills, 1; Cholera Morbus, 117; Cholera Infantum, 250; Cholera (epidemic), 2,082; Cerebritis, 5; Convulsions, 50; Congestion of the Brain, 33; Cramp, 11; Congestive Fever, 16; Cancer, 6; Catarrh, 4; Concussion of Brain, 1; Congestion of Stomach, 1; Diarrhœa, acute, 137; Diarrhœa, chronic, 54; Dentition, 28; Diphtheria, 2; Drowned, 2; Dysentery, acute, 46; chronic, 37; Dropsy, 5; Delirium Tremens, 1; Dyspepsia, 1; Epilepsy, 1; Eclampsia, 4; Erysipelas, 3; Encephalitis, 5; Enteritis, 6; Febris, 11; Gastritis, 11; Heart Disease, 1; Hemiphlegia, 1; Hemorrhage of the Lungs, 1; Hydrocephalus, 28; Hepatitis, 4; Hydrapisia Cerebri, 3; Inanition, 1; Intermittent Fever, 7; Inflammation of the Bowels, 9; Inflammation of the Brain, 42; Jaundice, 3; Lock Jaw, 2; Malformation, 1; Meningitis, 39; Nephritis, 2; Œdema Cerebri, 4; Over Distention of the Bowels, 1; Puerperal Fever, 1; Pleuritis, 1; Pneumonia, 19; Phrenitis, 1; Pregnancy, 2; Peritonitis, 4; Phthisis, 112; Polydipsia, 2; Paralysis, 2; Pertussis, 11; Premature Birth, 6; Poison by Opium, 2; Remittent Fever 12; Rheumatism, acute, 1; Still Born, 29; Syphilis, 1; Suicide, 1; Senectus, 1; Typhoid Fever, 92; Typhus Fever, 6; Tetanus, 3; Trismus, 4; Tabes Mesenterica, 5; Transformation of the Heart, 1; Unknown, 43; Ulceration of the Bowels, 1; Wounds, 6. Total, 3,517.

Nativity: United States, 2,231; Ireland, 592; Germany, 429; England, 54; Denmark, 1; Scotland, 11; Bohemia, 14; Switzerland, 3; France, 28; Unknown, 112; Italy, 12; Canada, 13; Norway, 1; Saxony, 1; Prussia, 8; Belgium, 4; Poland, 1; Holland, 2. Total, 3517.

Receipts of Subscriptions not otherwise Acknowledged.

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THE SAINT LOUIS

Medical and Surgical Journal.

NOVEMBER AND DECEMBER, 1866.

Original Communications.

TREATMENT OF CHOLERA BY ATROPINE AND SALINE SOLUTIONS.

BY PROF. JOHN T. HODGEN, M. D., ST. LOUIS, MO.

Aware that the time of the readers of your *Journal* has already been taxed by the theories and speculations of many writers in regard to the pathology and treatment of cholera, I can hardly bring myself to the task of still further consuming it; but will do so for just this once, first assuring them that I will be brief.

In sixteen cases of cholera, all in profound collapse, blue, cold, shriveled, and pulseless, some with rapid, irregular, and difficult breathing, I pursued the practice to be detailed.

I injected into the areolar tissue from one-sixtieth to one-thirtieth of a grain of atropine.

FIRST CLASS—FOUR DEATHS.

In four cases there was no perceptible effect; the patients all dying in from three to sixteen hours.

SECOND CLASS—TWO DEATHS.

In two cases, within an hour, the pulse became perceptible and distinct, the skin warm, even, red; one of these died in eight, the other in fourteen hours. No water was taken by the mouth or injected into the bowels in either of these cases. One case was that of an old man of dissipated habits; the other young, robust, and of good habits.

THIRD CLASS—THREE DEATHS.

In three cases reaction was pretty well established within an hour after the injection of atropine. In these cases, warm water containing common salt, was injected into the bowels every half hour, from half a pint to a pint at a time, and in no case was it discharged. Cold water was also freely taken by the mouth without a return of vomiting; the water was rapidly absorbed; thirst was allayed; the vessels were filled; the wrinkles left the skin; the sunken eyes became again prominent, and afterward there was no cramp.

These cases require to be mentioned separately: One of them I saw at 12 M., and immediately began the treatment. At 1 P. M. the pulse was distinct, the skin warm; during the afternoon the vessels filled; the skin became plump; the breathing much improved, though still frequent. I corded the arms and legs, and the breathing improved, but on removing the cords, the breathing was again hurried. This I repeated twice with the same effect, allowing the cords to remain half an hour each time. At 8 P. M. the pulse began to fail; the skin became cool, finally cold. She died at 9 P. M. The vomiting, purging, and cramps did not return. No discharge of urine.

In another of this third class of cases, the pulse came up in forty minutes after the injection of one-thirtieth of a grain of atropine, and the skin became too warm. Water was injected and absorbed, so that in twelve hours after I first saw her, she had

every appearance of making a rapid recovery; the pulse was very good, the breathing easy, the skin warm, though she had discharged no urine. In twenty hours after I first saw her, she became pulseless and cold, but without a return of vomiting or purging. I injected atropine; she again reacted, and soon a small quantity of urine was discharged; diuretics were administered. During the day she was flighty, and could not always recognize her children. Eighteen hours after the last injection of atropine she was again pulseless and cold, though there was no vomiting, no purging, no shriveling of the skin. I injected atropine a third time, and she again reacted. Gave injections of warm broth; ordered wine and beef tea; continued diuretics, as there had been only a small amount of urine discharged. She was now delirious, and began to sink again in fifteen or twenty hours after the last injection, and died seventy-six hours after I first saw her.

The third case of this class responded rapidly to atropine, and ten hours after, by injections of warm water, the pulse became distinct, the skin warm, the vessels full. The patient died twenty-three hours after I first saw him, having sunk rapidly for two or three hours before his death.

FOURTH CLASS—SEVEN CASES RECOVERED.

Seven of the sixteen patients treated with atropine recovered, six of them rapidly and completely. In one case of the six, the patient took by injection half a pint of the infusion of buchu, one ounce of buchu with half an ounce of common salt to the gallon, every half hour for twenty-four hours. During this time, though the tea was retained, she had three dark, bilious evacuations, having taken a large quantity of calomel. The kidneys acted freely, and twenty-four hours after I first saw her, no one would have supposed she had had cholera, so complete was the recovery.

The seventh case did not recover so promptly. This was an exceedingly sensitive, nervous woman, of strong will, and that *will* fully enlisted for her recovery; she determined not

to die. This case responded promptly to one-fortieth of a grain of atropine. Water was allowed as a drink, and warm water, and sometimes beef tea, injected into the bowels. The patient was very much nauseated, and there was frequent disposition to go to stool; not so much immediately after the injection of warm water, for this seemed to allay for a time the irritation of the bowel; but when she went to stool only a little mucus, with a small quantity of water was discharged, so that both by the mouth and anus, not one-tenth of the fluid taken was discharged. The bowels filled, the skin flushed, and lost its wrinkles. Twelve hours after the first injection, she was again pulseless. A second injection of atropine restored it. Beef tea injections, and as much as she could be induced to take by the mouth, was ordered. Nausea and disposition to stool continued. No urine was discharged. The patient talked vivaciously and extravagantly, like one in the first stage of uremic poisoning. Twenty hours later she was again pulseless. I injected atropine, and a third time the pulse responded; urine was discharged abundantly, and the patient recovered rapidly; so that on the seventh day from my first visit, she took a boat for Quincy, Illinois.

Remarks.—This last case, and that in class third, in which reaction occurred three times, are particularly interesting, since both responded readily to atropine; both suffered from uremic poisoning; both had repeated periods of depression; both, to my mind, showing clearly that the patients were still laboring under the choleraic poison, or under a poison the offspring of either the disease or the treatment. To my mind, it was clearly uremic poisoning, and was the legitimate consequence of the intense congestion of the kidneys present in this disease.

In this age of philosophy, we very naturally seek a reason for any special course pursued in the treatment of disease. Believing this question has already been in the mind of the reader, I will answer briefly. The prominent pathological changes observed in the bodies of those who have died of cholera, are: Congestion of all the abdominal viscera except the liver; congestion of the lungs; and, from the retarded flow of blood through these

organs, a damming of the blood in the right side of the heart. The ganglia of the sympathetic nerve are also congested, and in these, as well as other parts congested, the products of inflammation have been found. Here are precisely the conditions we would expect to find, if there was suspended action or paralysis of the sympathetic nerve; viz., a relaxation of the walls of the blood-vessels, particularly of those organs most abundantly and exclusively supplied by this nerve. Here, then, we have precisely what we have when the sympathetic nerve is cut in the neck, occurring in the eye, the ear, and the side of the face. A congestion, an increase of blood, an inflammation ensues, and if it continues long enough, the usual products of inflammatory action remain as records of these changes.

Now what shall we do to relieve this congestion? Give purgatives to draw off the fluids, thus relieving congestion? Use the lancet to diminish local congestion, at the expense of a circulation drained to the lowest living point? Give brandy to further increase the carbonaceous elements of a blood already poisoned, so that it can not be aërated in the lungs? Give carbonate of ammonia to still further contaminate a blood loaded with urea, the very material out of which ammonia is manufactured?

When a galvanic current is sent through the parts suffering from congestion, the result of paralysis of the sympathetic nerve, the vessels are stimulated to contract upon and repel their contents, and the parts which before were congested, are immediately relieved of their congestion, the temperature reduced, and the symptoms of active inflammation are dissipated.

Atropine diminishes the irritability of the spinal cord; atropine produces a rapid contraction of the engorged vessels of the conjunctiva; atropine stimulates the action of the sympathetic.

The injection of saline solutions into the bowels is sufficiently indicated by the loss sustained by the blood.

Now, I do not believe atropine will cure every case of cholera; I do not believe it will CURE any case of cholera. But I do believe its judicious use will so relieve the congestion consequent

upon the action of the choleraic poison, that remedies may be introduced into the system; that we are enabled to replace the lost serum of the blood by the best substances known to us; and that in cases where the poison has already been eliminated, and the patient is in danger of death from simple exhaustion, that this affords us an opportunity of supplying the waste, and thus saving the patient.

A FEW WORDS ON CHOLERA.

BY E. MONTGOMERY, M. D., ST. LOUIS, MISSOURI.

In a short paper on "Epidemic Cholera," read before the St. Louis Medical Society, in November last, and published in the *Journal* (April and June), I took the ground that opiates and astringents were not only useless, but pernicious remedies in the treatment of that disease; and that the administration of exciting stimulants internally, and the employment of hot applications externally, were, to say the least, remedial measures of more than doubtful applicability. I also intimated that these therapeutic views were in direct opposition to almost all who had written on the subject.

Since that time (November, 1865), I observe with pleasure that several physicians of note seem to doubt the propriety of giving opiates, astringents, or exciting stimulants, and some go so far as to condemn the practice *in toto;* among others, Dr. George Johnson, Professor of Practice of Medicine in King's College, London, and Dr. Braithwaite, the learned editor of *Braithwaite's Retrospect.*

In my humble opinion, Dr. Johnson goes to the opposite extreme in recommending exhausting emetics, as the tartarized antimony; and purgatives, as castor oil and salines. The only purgatives suitable, according to my experience, being mercurials,

and those in small and frequently-repeated doses; not with the view of purging, but as deobstruents to excite the secretions, keep up the fluidity of the blood, the equilibrium of the circulation, etc., etc. My favorite emetic is salt and mustard, and it can be given largely and frequently until the stomach is thoroughly washed out, without any danger of its prostrating the patient. I am sorry to see that a great many medical men of our own country are still wedded to the old popular practice of administering opiates, astringents, and exciting stimulants in epidemic cholera. Even the erudite editor of the *American Journal of Medical Sciences*, and his eminent co-worker, Dr. D. F. Condie, of Philadelphia, advise opiates, although they combine them with castor oil in the premonitory symptoms, and with blue mass in the confirmed stage of the disease. (See July [1866] number of the *Am. Jour. Med. Sciences.*)

Having acted as student and assistant to a physician who had charge of a large cholera hospital in Ireland, in the epidemic of 1832, and having practiced rather extensively in this city during 1849 and following years, I may be permitted to say, as an excuse for my presuming to indicate a course of treatment directly opposed to a vast majority of the profession, that I have had considerable experience of the effects of remedies; and so firmly and sincerely am I convinced of the pernicious action of opiates and astringents in the disease under consideration, that I am conscientiously desirous of imbuing others with the same opinion. I also feel less delicacy in advocating a different treatment from that hitherto so very universally employed, from the fact that the latter has been so very unsuccessful. We all know that one prime cause for the want of prompt and beneficial results of internal medication in cholera is the obtundity of the nerves supplying the organs of nutrition, assimilation, absorption, secretion, etc.; and it is equally well known that opiates and astringents are the very medicaments we would resort to did we wish to produce this condition.

In many of the common diarrhœas where the evacuations run off thin, of a rice-water appearance, and without pain, if we

attempt to cure them with opiates and astringents we utterly fail; and even if we succeed in arresting the profluvia for a time, it will soon return in a much more unmanageable form. In those cases of painless diarrhœa, when the stools are like cornmeal gruel, and devoid of bile, opiates and astringents will greatly aggravate the symptoms and retard the cure, producing nausea, thirst, tympanites, precordial oppression, and fever. Precisely the same untoward conditions will accrue from their employment in epidemic cholera. We should not try to dam up the rice-water fluid, but endeavor to prevent its exudation by eliminating the *materies morbi* from the system; restoring the natural secretions; arousing the dormant or semi-paralyzed nervous energies; keeping up a free circulation and equilibrium of the vital current, etc., etc.

A very small number of remedies may suffice; in the first place, a salt and mustard emetic; then small and frequently-repeated doses of some of the preparations of mercury, until bile appears in the evacuations; or until the vomiting and purging abate, an occasional dose of subnitrate or carbonate of bismuth, with creosote, and after these, frequent small doses of dilute nitric or sulphuric acid. Injections containing sulphate of quinine, sulphate of iron, and sulphuric acid, should be freely thrown up the intestines as long as the alvine discharges continue, as I believe these enemata powerfully tend to stop the serous exudations from the mucous follicles, and being in some measure absorbed, assist in re-establishing the normal vital functions. Of course, a good supply of pure, fresh air is necessary, and the patient should be allowed small quantities of cold water frequently. Whilst using the mercurial preparation, the cold water drinks may be medicated with soda and citric or tartaric acid, by way of effervescing drafts; and afterward the water may be acidulated by dilute sulphuric acid.

As before intimated, I am of opinion that warm baths and hot applications generally, are injurious in this disease; cold sponging with ice water, in the first stage, and afterwards assiduous and free rubbing with dilute water of ammonia, will be found of

essential service in relieving the cramps, abating the too copious cold perspiration, and promoting the capillary circulation. I have often witnessed wonderfully happy effects, in collapsed cases, of diligent hand-rubbing the whole surface of the body and extremities with dilute ammonia water. I believe that besides its vivifying effects on the nervous periphery, that some of it is absorbed and some inhaled, and the blood thereby rendered more fluid, and, consequently, better adapted to circulate freely and normally throughout the system.

In conclusion, Messrs. Editors, permit me to say that whilst many are beginning to doubt the propriety, and some to condemn the practice of giving opiates and direct astringents in cholera, I hope the day is not far distant when all respectable physicians will unite in discountenancing a system which has hitherto been so unsuccessful.

CHOLERA ASPHYXIA.

BY R. BLAND, M. D., ST. LOUIS, MO.

[Read before the St. Louis Medical Society.]

The cholera asphyxia, as observed in St. Louis and vicinity, more especially that form coming under my observation, has presented the same combination of phenomena that has distinguished this epidemic in other quarters of the globe; and when the varieties of constitution, age, sex, etc., are duly considered, there is a remarkable uniformity of symptoms, particularly in the asphyxiated stages.

The disease has been less malignant in this country than in Europe and Montreal, probably owing to better ventilation and greater cleanliness of our cities; for observation will assure us that the disease, at least in large populations, as in cities, requires for its production the coöperation of miasma with the

mysterious epidemic poison, the qualities and constituents of which have never been ascertained; and, doubtless, the same local developments intensify the poison in whatever sections or districts it may prevail. Cities, therefore, with spacious streets, with proper attention to cleanliness of houses, cellars, yards, alleys, etc., and in which too many individuals are not permitted to crowd together, may expect to reap a rich harvest of exemption from the terrible malignancy and fatality of the disease. And these hygienic measures are so vitally important as to demand the most urgent attention of the authorities of large cities, and a neglect of them ought by no means to be excused, dictated as they are by experience and philosophy.

The January number of the *Richmond Medical Journal* has a good article on the necessity of the utmost vigilance on the part of city authorities in the application of all the necessary agents to secure the ventilation of houses, and the cleanliness of streets, cellars, alleys, etc. An extract from it I will quote:

"Though cholera will prevail where there is strict cleanliness of place and person, its whole history teaches the imperative necessity of having municipal cleanliness strictly enforced. In Brussels, and Boston, and a few other cities, where there was a rigid enforcement of hygienic laws, the disease disappeared after a few days; whereas, at Calcutta, New Orleans, Havana, Tampico, and other places, where these precautions were neglected, the disease raged with great malignity for many months. * * * * * In a recent report by a committee on the present cholera epidemic, we are informed that 'in the progress of the disease, in no other place has there been any thing like the terrific ravages in Constantinople, notoriously the most filthy city in Europe.' The average duration of cholera in large cities is about ten weeks."

In St. Louis and vicinity, it may rather be said that we have suffered several endemics, than a general epidemic. The epidemic poison was not sufficiently active to produce a predisposition to the disease, without coöperation of local causes. It was more particularly confined to the malarious districts, both in the city

and country, and generally to such houses as were small, and crowded with inhabitants; in some instances, many have died in one house in rapid succession. Although the phenomena observed here are strikingly similar to those of Asiatic cholera, they are more or less distinct in different subjects; each, however, manifesting the same combination. The symptoms more liable to modification are lividness and collapse of the features; these are móre frequently absent than has been remarked in other countries. Asphyxia is a uniform symptom of the stage of collapse; so are the unequal distributions of heat, vomiting, and purging of rice-water fluid almost invariable; very generally there are spasms, cold sweats, corrugated skin and blueness of the fingers, cold breath and suppression of the secretions. So far as my observation has gone and medical *autopsies* declare, the morbid appearances have been less uniform than the symptoms, consequently our pathology has been much embarrassed. The small intestines exhibit some appearances of congestion, either of the mucous or serous coats, generally of the former; the large intestines are commonly pale, nearly white, particularly if the diarrhœa has been profuse; the stomach is nearly natural; the liver is sometimes greatly injected, but often with no appearance of departure from the normal state; kidneys, spleen, and pancreas natural; bladder empty, and often contracted. The gall bladder contains from one to two ounces of black, green, or yellowish bile. There is generally, but not always, absence of this material in the intestines, which usually contain a substance similar to that evacuated during life. It resembles, more than any thing else, rice-water, with flocculent matter, which is soon deposited.

The texture of the small intestines is softened; this is not always, but frequently so, which induced some physicians to ascribe this result to violent inflammation of these organs, and to regard this as the proximate cause of the disease. But so far as symptoms and appearances that usually denote inflammation are necessary to this theory, they are certainly nearly wanting in cholera asphyxia, venous congestion being almost the only

sensible condition of the blood vessels. Both cavities of the heart are sometimes filled with black blood, and they are sometimes empty, and these appearances also attend the lungs. The brain is more uniformly injected than any other organ, and usually contains, either in the ventricles or within its membranes, an unusual quantity of serum; and yet the integrity of the mind is not much impaired. The veins of the spinal sheath are also commonly full. In this city and country, as everywhere else, the disease was more malignant at its first invasion, and became milder as the epidemic atmosphere abated. Most of the subjects perished at first, and if the after success was greater, which, I believe, was the case, it was owing to the milder condition of the disease, to more contracted premonitory symptoms, and to the fact that the persons most susceptible to the epidemic influence had been swept away. The nature and treatment of the disease are perhaps no better understood now than when it first invaded Europe. At Quebec and Montreal scarcely any recovered after the accession of asphyxia; here, the disease having been milder, the recoveries after that stage have been more frequent—perhaps from one in six to one in ten. If the habits of the patients had been regular, perhaps one in six recovered. "But no such good fortune awaits the intemperate; the noise which he hears is his funeral dirge."

Our experience of the past, combining very great and varied resources, places us on higher and more confident ground, in regard to the more successful management of the terrible disease. To my mind, the functions of the body are nearly or quite suspended. The cerebro-spinal system, together with the great sympathetic, appears to be laboring under the influence of a powerful poison, and either fails to supply healthy nervous influence to the various organs of the body, or the supply is so irregular as to break up the harmony of action. The functional performance of these organs is partly or wholly suspended; in consequence, we see the several phenomena already mentioned.

It will not do to depend upon any particular remedy or combination of remedies as a panacea for this disease. The enlight-

ened physician should be full of resources to meet the enemy at every point of attack; in the application of these resources, he must be guided by the conditions and indications presented to him. Thus armed and equipped with resources, and the power of discriminating real pathological conditions, he enters upon his work, hoping to moderate the force and lessen the fatality of this formidable plague. Should the disease come on gradually, there being but little disturbance of the system, other than a diarrhœa, which at first may have been dark-colored and liquid, but soon assumes the large rice-colored discharges which persist more and more,—the physician should lose no time; if possible, the disease should be arrested before it enters a more alarming state, which it will certainly do unless timely measures are used to arrest it. I know of no better treatment of this premonitory diarrhœa, than the use of calomel in small doses, either with or without opium. Five grains of calomel may be given every two hours until the operations are changed, become more consistent, and of a yellow or dark green color. Should there be cramps, attended with slight fainting sensations, with no approach to coma, combine with the calomel from one fourth to one half grain of opium, and one grain each of camphor and capsicum; in conjunction with this treatment, apply sinapisms to the whole spine, upon the extremities, and a large one upon the abdomen. In fine, all means necessary to promote secretions, and distribute nervous and vascular equilibrium, should be perseveringly employed. Should the treatment just stated be instituted at an early period, a large majority of such cases will recover; but should a more malignant form be developed, it will be attended with deadly aspect of countenance, bodily functions nearly or quite suspended, stomach not sick but every now and then ejecting its contents all over the bed, and sluicing the floor by a sudden spasm of the organ, profuse diarrhœa, and frequent operations of a rice-water character, attended with spasms of the abdominals and muscles of the extremities.

I would not advocate large doses of opium in such cases; coma may be expected at any moment, and opium only tends to hasten

it. In such cases one grain of calomel and one of extract of hyoscyamus given every ten minutes, aided by the following combination, given in teaspoonsful doses every half hour, hour, two hours, or three, as the urgency of the case may demand, may offer some hope, though slight, of relief:

Aquæ Camph.
Aquæ Am.
Ess. Peppermint
Spts. Lavander, *a a* ʒi.

In all the grave forms of the disease the features are altered, eyes sunken, skin corrugated and shrunken, lips and fingers livid; the surface is cold and damp; there is cold breath and much sighing; the pulse is slow and laboring,—falls down from eighty to seventy, sixty, fifty, in a minute. These cases may not be attended by much nausea or vomiting, and the purging may be slight; not sufficient by any means to account for their rapid termination, or for the significant change that has seized upon the whole aspect of the patient. In a few minutes such patients are transferred from athletic health into the conditions and phenomena just related. In many of these cases the patient appears to be laboring under a powerful poison. Commonly, while the organic functions appear nearly suspended, the intellect is very steady; in such cases the acuteness of the intellect is remarkable. I can not advance any treatment that offers much promise of good results. I have seen few if any recover. Still no effort should be spared to restore equal action. All internal and external agents should be employed to overcome the deadly impress of the mysterious poison upon the nervous, vascular, and organic system. In such cases I would give calomel in small doses, combined with opium, camphor, and carbonate of ammonia. The calomel should be repeated in five-grain doses every half hour. I have no experience in the use of chloroform, yet from my knowledge of its effects I would advise its use. But the remedies particularly indicated are stimulants internally: viz., carbonate of ammonia with brandy; ammonia water with paregoric; peppermint and cinnamon water, and calomel in small doses, at short

intervals,—while the external treatment, equally important, consists of all the means requisite to induce action upon the surface of the body: viz., mustard applied to the abdomen, extremities, and spine; bottles filled with hot water to the feet; strong rubefacients applied to the surface of the body. Although the prognosis is unfavorable from the beginning of such cases, we should by no means fail in the use of all our resources. We can not tell when reaction may occur. No matter how hopeless the case may be, all the agents with which experience has armed us to facilitate reaction should be used as long as life lasts. I now simply mention, in the way of trial in such cases, what has been suggested to my mind in view of what has been said with regard to the coöperation of miasm with the mysterious epidemic poison in the development of the disease. If the theory mentioned be true, can we expect to treat the disease successfully without the use of the only remedy so eminently successful in the cure of miasmatic fevers? Hence I have determined in my own mind to use, as my judgment may direct, the sulphate of quinine. I have determined to combine it with calomel, and, as circumstances may indicate, with opium. Many of the phenomena seen in congestive chills, or pernicious fever, are to be seen in cholera, and we all know how successful quinine is in such cases. May it not be equally so in these terrible cases of cholera?

A NEW CANULATED NEEDLE FOR INTRODUCING THE WIRE IN OPERATIONS FOR VAGINAL FISTULES.

BY GEO. SYNG BRYANT, M. D., ST. LOUIS, MO.

In reviewing the history of the operations devised for the cure of the various fistules connected with the vagina, it is surprising to see what little progress has been made in the invention of suitable means for introducing the sutures to hold the lips of the

opening together. In the early part of the seventeenth century, the treatment of fistules of the vagina by freshening the edges of the opening and uniting them by suture, was proposed by Roonhuzse, but that operation was attended with such unfavorable results, that little encouragement was given to surgeons to continue their efforts for a radical cure.

Desault, Liston, Hamilton, Davis, Vidal, and others, have all, in their turn, failed of success in operating for these fistulous openings. For a long time all efforts to cure by operation were entirely abandoned, and the unfortunate sufferer given up to drag out a miserable existence in retirement the remainder of life.

Credit is awarded to Prof. Naegele, of Heidelburg, for the revival of this operation, but the Professor, like his predecessors, made no improvement in the suture—using silk to bring and hold the freshened lips of the fistule together.

He, too, like those who had preceded him, had the mortification to see his efforts unsuccessful; and it is altogether likely, that if the wire suture had not been substituted for the silk, the operation for vesico-vaginal fistules would have ever remained a failure.

To Dr. Marion Sims, formerly of Alabama, much is due for his valuable services in the operations for fistules. And since the introduction of the metallic suture, the operation is attended with almost universal success when skillfully performed. This operation is justly regarded as one of the modern triumphs in surgery. Is there really a greater boon to mankind than the perfection of that art which can restore the suffering, invalid, bed-ridden mother to health, usefulness, and happiness in her beloved family circle? There have been a

number of needles invented for carrying the thread, but all have been imperfect, barely subserving the purpose; requiring much time and labor to introduce the wire.

It has been only a few years since Mr. Startin, of London, conceived the idea of a hollow or canulated needle for introducing the wire in vaginal fistules. The invention of this needle has greatly facilitated the operation, and has supplied a want much felt by the surgeon.

I have modified the shape of Mr. Startin's needle, and added to it a slide with a key and spring attached, so as to hold fast the wire, and furnish just the amount wanted at each stitch.

The accompanying cut is of two-thirds size, and requires no explanation, for with even a superficial examination, every part of the instrument will be readily comprehended by the surgeon. I am now having made a needle, curved laterally, for the transverse suture, in longitudinal or oblique fistules. I have also just completed a diagram for a needle upon this plan, to be used in operations for cleft palate.

Correspondence.

LETTER FROM EUROPE.

[Extract from a letter to Dr. Frazer, from his son, S. H. Frazer, London.]

LONDON, May 6th, 1866.

I have seen SIR WILLIAM FERGUSON operate five times, and I think he is wonderful in surgery, so far as cutting is concerned; he handles a knife very skillfully, and with such a manner of ease and confidence. You do not see any of the hurry and bustle that attends an operation in America. The table in the Amphitheater is covered with the instruments to be used, and with those it is possible they may want. The patient is brought in, and the

chloroform is administered by one of the assistants, who does nothing else during a certain period; another has the ligatures; a third the instruments; a fourth the sponge, etc. Every thing is quiet and systematic. Sir William took a review of the case after the operation was completed, telling when he would or would not operate, and then gave the steps of the operation, and the probable result. I like his manner very much; he seems very pleasant and sociable, laughing and talking with the students and his admirers, in the shape of surgeons, who flock to the hospital on operating days. He is, undoubtedly, a man of very great ability, having been knighted for merit in the profession, and is, I believe, generally conceded to be *the surgeon* of the world. Mr. Paget, the next surgeon of the city, I have not as yet seen. He is said to be a better surgical philosopher than Sir William, but none can surpass the latter in the use of the knife. The popularity is about equally divided. Mr. Paget is giving Dr. Hodgen's splint a trial,—has had one made and applied, and the house surgeon at St. Bartholomew's Hospital, where the splint is being tested, speaks well of it, and thinks it will be a success.

We went to see Mr. Errichson operate, but arrived too late to see the operation, and just in time to hear him lecture. I must say, I thought the production a very poor one for a man of such eminence. The language was good enough, but the delivery was bad.

This is also true of Charles Dickens, whom we heard read one night last week. He writes excellently, but can not read even tolerably well. His articulation is poor. His voice sounds as though it came from his chest, and I confess I was disappointed in him.

We went to see J. Baker Brown, the obstetrical surgeon, and found him accommodating, courteous, and kind, using his best endeavors to make the visit a pleasant one. After he had performed several operations, he conducted us to a room where he had a case convalescing from the operation of ovariotomy. This operation he has performed, according to his method, twenty-six times, with only three deaths. The peculiarity of his method is

in this, that instead of applying ligature to pedicle and cutting away the tumor, he burns the tumor off with caustic, and returns stump without ligature into the cavity, and permanently closes the external opening. He performed an operation, one original with him, three times the day we were at the hospital, which consists in the removal of the clitoris, which results in the cure of certain forms of epilepsy, insanity, catalepsy, and hysteria in females. These certain forms he attributes to peripheral irritation (brought on by abnormal practices) of the pudic nerve, especially of that branch of the nerve which is distributed to the clitoris. The operation, if generally as successful as in J. Baker Brown's hands, will be one of the greatest blessings that has dawned upon mankind for many a day.

We saw the great ophthalmic surgeon, Mr. Critchett, the one who originated iridectomia; and now he has another very simple but ingenious operation for treatment of vascular ulcers of the eye, by the seton needle forming an issue in the temporal region. He performed two operations for soft cataract, one for hard cataract, and two for divergent strabismus. I can not describe to you how delicate and nice the manipulation and cuttings were in these several cases.

I have not much to say respecting the hospitals. Some are very good, while others are very indifferent in structure and management. There are twenty-five or thirty hospitals in the city and suburbs; we have not visited all, nor do we intend to do so, but will confine ourselves to the principal general hospitals. We visited Charing-Cross Hospital the same day that we were at the Royal London Ophthalmic Hospital; did not visit the wards, but remained long enough to see Mr. Hogg perform two operations. King's College Hospital, not prepossessing in external appearance, surprises you with the clean, systematic appearance of things in the interior. Sir William visits and operates here. Todd, the physiologist, was connected with this hospital, and a statue of him stands in the building. St. Bartholomew's Hospital was founded in the year eleven hundred and two (1102), by Rahere, and gives relief to *all* poor persons suffering from disease

or accident, without other recommendation. Here, in sixteen hundred and nineteen (1619), Harvey first lectured on the circulation of the blood. Statues of Harvey, Abernethy, and several prominent men of the profession are in the museum. In the building there is a stair-case painted by Hogarth. Watson's lectures on the Practice of Medicine were delivered in this hospital, and it is now the field of Mr. Paget's operations. The number of patients treated in this hospital during a year is about seven thousand, and the clinical patients number seven thousand more.

Mr. ERRICHSON operates in the University of London Hospital. There are many more hospitals, and all are crowded. The field for learning is fine, but expensive. Fees for medical practice of St. Bartholomew's, for six months, are twelve guineas; and for surgical practice, fifteen guineas. S. H. FRAZER.

Transactions of Societies.

PROCEEDINGS OF THE ST. LOUIS MEDICAL SOCIETY.

REPORTED BY R. S. ANDERSON, M. D., RECORDING SECRETARY.

[Continued.]

DR. LINTON said, that he hoped that the discussion would not be entirely without interest to the Society. It being on pathology, it could not be unprofitable. The fifteen propositions he had written about a month ago, intending by them to express his views on General Pathology. He could not now discuss them in full, but was pleased to find that Dr. Hammer so nearly agreed with him. This was more than he had expected. When he commenced, he stated that one of the propositions was self-evident, and the rest were false; afterwards, if he remembered, Dr. H. said that nearly all of them were true.

At present he wished only to call attention to the misunderstanding which arose regarding his charge that Virchow misunderstood the doctrine of counter-irritation. He maintained that by counter-irritation, Virchow meant direct irritation; irritation of the part already irritated. Dr. Hammer at that time accused him of misrepresenting that author, and he procured the book and read the passage in which he used the language referred to. In this case Virchow evidently meant pathology, as well as physiology. Dr. Hammer stated that he referred to physiology alone. How is it possible for irritation to apply to physiology alone? Congestion is always pathological.

Again: It is bad treatment, as well as bad use of terms, for the dilatation of the vessels was originally produced by irritation. The dilatation follows the contraction in consequence of the exhaustion of the vessels, for the doctrine of active dilatation has been long given up. How then could additional irritation be the proper remedy? It would only produce slight contraction, to be followed again by greater dilatation than before.

Dr. LINTON, before proceeding to answer the objections of Dr. Hammer to his fifteen propositions, referred again to the subject of counter-irritation as held by Virchow, and read again the extract from that author, previously referred to. Dr. Hammer admitted the truth of the first proposition, saying that it was self-evident. He therefore passed on to the second proposition without any further remarks. He did not remember whether or not the second proposition, concerning the definition of irritation, was contested. He believed that Dr. H. had stated that it was true as far as it went. He maintained that depressants arrest the vital processes, and irritants increase them. He did not think it necessary to say much concerning the third proposition, only that it was *not* taken from Virchow. "Fluid" does not always mean blood, but he would go so far as to say that all such agencies increase the flow of *blood* to the parts affected. In extra-vascular parts, as the cornea, there is no red blood, but still there is a white blood. This fluid flows at first on an irritation of the part, and soon there is a flow of red blood to the part. When there is

no red blood in a part, but only these nutritive juices referred to, no red flows immediately, but this eventually occurs.

At this point he was reminded of what might be considered as an anticipation. Dr. Hammer asked how could a dyscrasia be transmitted from father to child, when there is no blood in the embryo at a certain period of its existence? There is a fluid contained in the cells which is derived from the blood of the father. What difference does it make if, although there is no blood, there is a fluid containing poison derived from the blood? This third proposition, as he had before remarked, was only an enunciation of the old axiom, "*ubi irritatio ibi fluxus.*"

In the fourth proposition, concerning exercise and repercussion, exercise might have been left out, as it only included the idea of irritation. Exercise may even cause inflammation, as in the case of mania produced by an over-tasked mind. By repercussion is meant the driving away of the blood from a part, producing congestions elsewhere, as by cold, etc. Thus we apply ice to the head to relieve congestion there, and in bandaging the extremities produce internal congestion, and perhaps even inflammation. It may be contested that inflammation can be produced in this way, and it is disputed by Virchow, who admits, however, that a determination of blood, not preceded by local irritation, may produce heat, redness, pain, etc.

In the fifth proposition, by "fluxion" is meant a determination of fluids to parts. He meant to say that there is no inflammation without a determination of fluids to the part. It is, therefore, a *sine qua non.*

In the sixth proposition he contended that a little fluxion merely produces an enlargement of the part. When we exercise a part it merely becomes hypertrophied, and not inflamed. If we withdraw the fluid again, it becomes atrophied. On this point he could quote Virchow, who says that an increase of the cells of a part is the first step of inflammation.

The seventh proposition is very much like, but not the same as the sixth. Hyperplasia is one form of hypertrophy. He did not in this proposition, choose to discuss the two theories of cell

production; one, that of Virchow, that no cell arises from a blastema, and the other that cells do spring up in a hyaline blastema. Both of these theories are prevalent, therefore he had admitted both. It amounts to this, that exudation and adhesion follow hypertrophy in this process. One idea is that the cells are formed into fibre and stitch parts together; the other that fibres themselves arise from the blastema and perform this function.

In the eighth proposition, concerning suppuration, he did not maintain that nothing else could occur between hyperplasia and suppuration. He did not design to mention and describe every step in the process, but had only stated that this was a further step. He did not recollect that there had been much discussion concerning this. Suppuration is nothing but the multiplication of cells which are changed in some way, perhaps, by superoxidation, so that instead of being organized into tissue, they are converted into pus. It is a product of advanced inflammation. Here he stated that he ended the discussion of the process of inflammation, and proceeded to discuss other pathological subjects.

In the ninth proposition, it is stated that these hyperplasias or exudations, either term being used, differ in quantity—of this there can be no doubt—and also in their quality. It is hardly necessary to argue this, as it is admitted that a solid may be altered in quality as well as a fluid. These, it is further stated, constitute nearly the totality of the so-called deposits and growths. We can not fancy more than three of these: hypertropy, atrophy, and cacotrophy. Among the deposits, tubercle is admitted by every one to be mal-developed cells arrested in the lungs. The malignant and non-malignant tumors differ in quality. He did not think that there were any changes in these formations which did not depend on these conditions.

In the tenth proposition it is stated, that when these cells are of low vitality they constitute tubercle. He recollected, ten years ago, having advocated the doctrine of the cellular nature of tubercle. When they persist,—have a higher grade of utility, but finally decay and die, they constitute cancer. This was his own view of the difference of tubercle and cancer, and he thought

that there was every grade between the two. When these cells possess sufficient vitality to become fibrous tissue, they form benign tumors. Therefore, according to the quality of the neoplasm—the amount of life in the cells—they produce tubercle, cancer, or benign tumors. Can a tumor, first fibrous, be afterward converted into cancer? This may be so, for though the plasma may have had sufficient vitality to become organized, it may afterward degenerate through depravity of the system, and be converted into cancer.

Concerning the curability of cancer, he stated that of course some cancers were capable of cure, there being every degree of cancer from the almost benign to the most malignant. The milder forms may, of course, be cured.

Concerning Dr. Hammer's statement, that there are some malignant tumors which are not cancers, as, for instance, the medullary sarcoma; he said that he considered all malignant growths to be cancerous, the medullary sarcoma being one of the most malignant.

No other objection was made to the eleventh proposition than that the terms used were not sufficiently scientific. He would like to ask the gentleman to give better names than those he had used: Aplastic, signifies no formation, or very little; Cacoplastic, bad formation, varying from the standard of health (it is not a definite term, and does not indicate the different classes of these tumors); euplastic, good formation. These terms are in universal usage, and express as well as possible the idea he wished to convey.

In the twelfth proposition it is maintained, that if the fluids are healthy, the formations will be healthy. If the fluids are of low vitality, then the new-formations will be cacoplastic. These, therefore, depend upon the previous condition of the fluids. Whether or not there be a dispute concerning the amount of the influence which the fluids have, there can be no doubt that there is an influence. Has it as much to do with the new-formations as I have said? Dr. Hammer read from Virchow that these dyscrasias were produced and kept up by local diseases. The

deposit or growth being caused by quality of the blood, may in turn, of course, infect the blood.

The dyscrasia most concerned in phthisis was, in his opinion, that of anæmia. This he had taught for many years, and it is advocated by Chambers in his last work. All that he wished to say was, that these previous conditions of the blood had much to do with the quality of the deposits.

The thirteenth proposition was admitted to be true. Dr. Hammer stated that the doctrine of Lebert and Robin had been overturned by Virchow. It was previously overturned by Andral, and he himself had combatted it.

In the fourteenth proposition, he contended that these so-called new-formations were merely normal cells, changed as to time, place, etc.

Dr. WATTERS, having the floor, occupied the attention of the Society for the remainder of the evening. We give, at present, only a brief synopsis of his speech. He remarked, in reference to the positions of Drs. Hammer and Linton,—the one advocating the Cellular Pathology of Virchow, and the other the Humoral Pathology,—the one claiming that the cell is the seat and source of all vital change, and the other referring these changes to the blood,—that the truth seemed not a middle course between these two, but rather such a union of the two, that the facts of the one should not be exclusive of those of the other. To present the condition of cell life, he entered into the history of cell generation, and attempted to show that the cell was only a "moment" of life, and not independently endowed with vitality. That vital force and vital phenomena originated only when the cell was placed under certain conditions,—that it was to be referred neither to the cell nor to the blood; neither to the ovule nor to the spermatozoon, but to the two united, under certain specific circumstances.

As a deduction from this position, he argued that disease could not be primarily an alteration of form, either of the cells or of the blood, but that this alteration is only a "moment" of disease, as the cells and blood are only moments of life. He then

remarked that his object was only to develop, as clearly as possible, these fundamental principles of life and of disease, and that he would leave it to the individual members of the Society to make the application to the special subjects before introduced into the discussion, and to all subjects of pathology.

Dr. HAMMER, having the floor said: Dr. Watters, at the last meeting, stated that he was the advocate of the Cellular Pathology of Dr. Hammer, with the Humoral Pathology of Dr. Linton. He contended that the position of Dr. Watters, as stated by himself, was also his own. He had maintained the blood to be a tissue as well as the solids, and had never advocated the exclusive doctrine of solid cellular pathology. He therefore regarded their differences more those of words than of ideas.

Dr. Linton had stated that since investigating the subject lately, he had become one-half a disciple of Virchow; and he also stated, that he was glad to find that we were "so nearly agreed concerning the doctrine;" in other words, that he had adopted Dr. Linton's own views. In the beginning of the discussion he had stated that he did not propose to discuss only the views of Virchow, as given in his late work on pathology, but meant to review all the discoveries connected with the subject up to the present time. Dr. Linton has spoken only concerning the views contained in Virchow's Cellular Pathology, published in 1856.

Let us review the cardinal points of difference: The fundamental principle that characterizes Virchow's Pathology is the doctrine that there is no such thing as "free cell development." On the contrary, Dr. Linton has always been the advocate of this, but now says that cells may be developed in both ways. Again: Virchow holds that the first change in the process of inflammation is in the cells themselves; that hyperæmia and congestion have nothing to do with it. On the contrary, Dr. Linton says that hyperæmia and congestion is the first step in the process. The third great point of argument is concerning the doctrine of dyscrasias, and the specific character of certain pathological formations. These Dr. Linton mixes up sadly. One of the

most fundamental, is the doctrine of dyscrasias; Dr. Linton holding that all dyscrasias are founded on primary disease of the blood, Virchow contending that they all depend primarily on local disease, and that the blood poison is secondary to this. In the face of these essential and radical differences of doctrine, Dr. Linton says that he is one-half a disciple of Virchow, and that he (Hammer) had come round to his views. He was represented by Dr. Linton as having said that his first proposition was self-evident, and the rest false. Dr. Linton forgot to mention that he had stated, that what was original in those propositions was not true, and that what was true in them was taken from Virchow. He then returned to his favorite scheme, assuming a fictitious position, and discussing a subject not at all relevant to the point. He criticised Virchow's remarks concerning counter-irritation. On this point he intended to show that Dr. Linton had criticised himself. Virchow says that when a vessel is irritated it contracts, then dilates, and remains so. He then says, that in order to restore it it must again be irritated, and this is what he calls counter-irritation. This practice Dr. Linton called irrational. At that time he had recommended Dr. Linton to study physiology, and he would now explain why he made the remark. Let us irritate a vessel; it contracts, then dilates; the sympathetic nerve is paralyzed. This state of dilatation favors inflammation. Virchow recommends the application of an irritant to recontract, and this Dr. Linton could not understand. He forgot that besides the influence of the sympathetic nerve, there is also an inherent quality of contractility in the vessel itself. If the sympathetic in a rabbit's neck be cut, it is followed by dilatation of the vessels of the ear. Let ice be applied, the vessels contract; not from the nervous influence, for the nerve is cut, but from the inherent organic contractility of the vessels themselves.

Dr. Linton, doubtless, adopted this practice in the treatment of gonorrhœa, when he applied such agents as nitrate of silver, or sulphate of copper, or zinc, to a urethra already irritated and dilated by the gonorrhœal veins.

Dr. LINTON here inquired, if the first irritation dilates, will not the second increase this by further exhaustion?

Dr. HAMMER answered, that the contractility of the vessels depends on the conditions—the direct nervous influence, and the organic contractility of the parts themselves. Further irritation acts on the latter function.

This is not all. He blames Virchow for a wrong use of terms in calling this counter-irritation. If this be an error, he himself is guilty of the same, when a little further on he uses the word hypertrophy in a very unusual sense. By this term he characterizes the little swelling of the cells, which is only discoverable by the microscope. Is this not a similar error to that with which he charges Virchow?

He had stated at a previous meeting that a disease might be transmitted from father to child through the semen, which contained no blood. Dr. Linton said that he had caught him, since he had admitted that the fluid was derived from the blood. He maintained that the blood was a tissue, differing only from other tissues in the fluidity of the inter-cellular substance. Why should the blood be the only tissue by which disease is propagated, when this is a tissue like the rest? He had been far from saying that blood could not convey disease. He will not dispute that when the organism is originated, it has no blood corpuscle in it. When we see this, can we not conclude that the first animal came into existence in the same way? We are forced to admit this, both by logic and reason.

In commenting on the fourth proposition concerning exercise and repercussion, Dr. Linton admits that cold acts so as to contract tissues, and says that repercussion in this way produces congestion and inflammation. Here, again, hyperæmia is the *sine qua non* of inflammation; while, in another place, he had already said that inflammation began in the cells. This shows the ambiguity of his views.

In the proposition concerning hyperplasia by exudation, or free cell development, after going through the various processes of inflammation, he admits that he knows nothing about it.

He had been asked by Dr. Linton to explain his objection to the terms, "aplastic, euplastic, and cacoplastic." Aplastic is only a formless thing, merely the serum of an edema or ascites. It is not a growth. There can be no tumor of this character, unless water is a tumor; and then it is not aplastic, for it must be surrounded by a membrane which contains cells. Cacoplastic —a formation that kills—a cancer. A fatty tumor of the heart will kill. Would that be cacoplastic? The euplastic is formed from healthy blood, and transformed into fibrous tissue. Suppose a fibrous tumor in the larynx, it would kill. Would it be euplastic? Concerning the specific cells of cancer, he had often explained that this depends on hetero-topic or hetero-chronic cells.

REMARKS ON PUERPERAL FEVER.

Dr. M. M. PALLEN said, that during the last two years he had heard many references made by members of the Society to the subject of puerperal fever, and many different views had been expressed. He had not participated in these discussions, as he was not fond of talking, but since there seemed to be some indisposition on the part of others to speak to-night, and as it was not desirable that the hour for discussion should pass unprofitably, he would offer a few remarks on this subject.

Is there really such a disease as a fever peculiar to the puerperal condition, and not witnessed in any other? There are differences of opinion on this subject, and those who admit it by no means agree as to its pathology.

There are many who regard puerperal fever as a fever resulting from local inflammation, whether it be peritonitis, metro-peritonitis, or inflammation of the uterus and its appendages.

Those who agree that it results from local inflammation, are not agreed as to the cause of such a condition; some contend that it is owing to a vitiation of the fluids caused by a morbific agent entering into the blood by contagion, or by absorption of putrilage contained within the uterus; others say that such inflammations are due to other causes,—to cold, to protracted and

tedious labors, etc., and are nothing more than similar lesions witnessed in conditions non-puerperal.

Again: It is asserted, that although such inflammations are occasionally met with in puerperal fever, they are not the causes of the fever. They are rather the effects, or are coincidences, the fever being an essential one, depending on the puerperal condition, and observed in none other.

Some assert that it is contagious; others deny it. His own belief is, that there is an essential fever peculiar to the child-bed condition; that such a fever, under certain circumstances, may be propagated by contagion, carried from bed to bed, or from ward to ward, as in a hospital, or by the accoucheur from house to house,—contagion being one cause. Another cause, much oftener occurring, is the absorption of putrilage contained in the uterus, which, immediately after parturition, has well been described as resembling the stump of an amputated limb; and when such a fever occurs, it is often, very often, accompanied by an inflammation, more or less sthenic, sometimes asthenic. He also believed, that although such cases do occur to us in private practice, we generally have to deal with inflammations (a peritonitis, or a metro-peritonitis) similar to such lesions occurring in other conditions non-puerperal.

These inflammations arise from the patient taking cold, or from a protracted, and to use an expressive term, laborious labor; and in proof of this, he would state that he had seen, as others have seen, the inflammation commence before the birth of the child, and, consequently, before any absorption could take place. We have, therefore, continued Dr. Pallen, in treating fever after parturition, to look to the symptoms of each case. If it be an inflammation (a peritonitis or a metro-peritonitis), treat it as such. If it be of the sthenic type it will require the use of the lancet, calomel, opium, etc. Venesection, according to his opinion, has lost none of its value, notwithstanding the present *fashion* of therapeutics. In opium, particularly, if combined with calomel, he had great confidence, but he could not approve of the enormous doses recommended by Prof. Alonzo Clark.

Whilst he (Dr. Pallen) thus adhered to the old remedies when they were indicated, he by no means would be understood to say all cases required such heroic treatment. On the contrary, many required stimulants, and some mild cases required little or no medication. He wished to impress others with these views: The fevers after parturition are of various types—they arise from various causes—they present different symptoms—they are to be treated as those symptoms indicate their pathology, whether by active depletion, or by stimuli, or on the expectant method.

PROCEEDINGS OF THE SHAWNEE COUNTY MEDICAL SOCIETY.

The first Quarterly Meeting of the Shawnee County Medical Society convened at the office of Drs. Stormont and Bailey, in Topeka, Kansas, at 2 o'clock P. M., October 3d, 1866. The meeting was called to order by the President, Dr. Bailey. The minutes of the preceding (called) meeting were read and adopted.

Papers on the following subjects were read before the Society: Report of a case of Traumatic Phlegmonous Erysipelas, by Dr. Bailey; report of a Post-mortem in case of Death caused by taking into the Stomach ℥ij Chloroform for the purpose of committing Suicide, by Dr. Stormont; report of a case of Intestinal Obstruction, by Dr. Campdoras; a paper on Hypodermic Injections, by Dr. Brown.

Dr. Teft, Chairman of Committee on Fee Bill, submitted a report, and the following resolution, which was adopted:

Resolved, That the rule which enjoins the gratuitous services of our profession to actual clergymen and their families, shall not apply to those, who by their influence or name sanction quackery.

On motion, the Secretary was instructed to forward a copy of the proceedings of this meeting to the *St. Louis Medical and Surgical Journal*, for publication.

Adjourned to meet on Wednesday, November 14th, 1866, at 2 o'clock P. M.

S. CLAY BROWN, *Secretary*.

The following is the case reported by Dr. Campdoras:

D—— B——, a well-to-do farmer, about forty-eight years of age, of good habits, a strong constitution, large chest, and well-developed muscular system, while feeding cattle in a yard covered with snow, on Saturday morning, December 9th, got his feet damp, became chilly, and felt rather unwell. Yet he took breakfast with his usual appetite, but soon afterward was attacked with violent pain in the abdomen, nausea, and in a little while vomited the contents of his stomach, and also stercoraceous matter. About ten years before, he had been affected with intestinal obstruction, which, after ten days of great distress, and when he was supposed to be in a hopeless condition, yielded to the use of croton oil. Since that time he has had four or five similar attacks, and has always been relieved by the use of the warm bath, and active cathartics. This time, however, the same remedies were employed without any effect.

On Sunday, December 10th, I was called to see the patient, and arrived at his residence at 1 o'clock P. M., about thirty hours after the commencement of the attack, and found him in the following condition: Dorsal decubitus; face expressing great suffering; at times quite restless, and at other times tolerably easy. A constant, but not very severe pain was felt on the left side of the abdomen, about two inches from the umbilicus; and every few minutes the pain at this place became very violent, lasting only a short time; pressure with the hand not very painful; percussion giving the same sound all over the abdomen; no signs of any tumor, and very little tympanites; no evacuation of the bowels since the attack, but gases are frequently expelled by the mouth, and many times, especially after drinking, vomiting of stercoral matter takes places. Before the attack the bowels were regular; the breath had a stercoraceous smell, the tongue moist, and a little whitish; the pulse seventy-five, and of average fullness. The patient did not appear alarmed, and believed that active cathartics would remove the obstruction as they did before. Prescribed podophyllin, three grains, to be repeated in eight hours if necessary; extract hyoscyamus, two grains every four

hours; enema, with sulphate of magnesia; warm bath and warm fomentations, with decoction of hops.

Dec. 11*th*, 9 *A. M.*—No evacuation of the bowels; pulse eighty-four, full, and hard; vomiting more frequent; dysuria, and thirst; the pressure is more painful; the paroxysmal pain in the bowels more frequent; more tympanites, and the patient not so cheerful. Prescription: Venesection, ℥xij; calomel, grs. xv; opium, grs. iij, in twelve powders, one to be taken every two hours; and continue bath, enema, and fomentations as before.

After having taken three doses of calomel and opium, the patient slept about two hours. At 5 P. M. he awoke, and immediately commenced retching and vomiting, almost incessantly. The pain in the abdomen was so intense that the patient was bathed in cold perspiration, had lost the power of speech, and was frequently on the point of fainting, being hardly able to execute any movement. The attendants thinking he was dying, stopped the administration of medicines. At 10 o'clock P. M. there was a change for the better, and the powders were given again.

December 12th, at 1. P. M., when I saw him, vomiting had ceased; gas had frequently been expelled per anum; urine voided without trouble, and the patient in good spirits, believing himself out of danger. Tenderness yet on the left side of the abdomen, but no pain; yellow coloration of skin; pulse seventy, and of average fullness; no tympanites; tongue moist, and not so much thirst. No evacuation of the bowels yet,—perhaps there is nothing to evacuate besides gas. Nine of the twelve powders had been taken; allowed beef tea.

Dec. 13*th*, 11 *A. M.*—Patient was able to sit up while his bed was being made; had a small alvine evacuation in the morning; no more vomiting, but some little retching; pulse sixty-six; the gas and breath have a stercoral odor yet. Calomel and opium powders, one every six hours; enema of sulphate of magnesia every twelve hours; appetite good. Diet: beef tea, soft eggs, and wine.

Dec. 14*th.*—Improving steadily; pulse, sixty-six; had two passages of the bowels; no stercoral odor of breath. Extract of hyoscyamis two grains at night. Dismissed the case.

Since that time the patient enjoyed excellent health until I was called to see him again, August 15th, at 2 P. M. The morning before, being a few miles from home in a wagon, he became chilly, and felt generally unwell; toward evening commenced vomiting, which became more frequent during the night, but there were no signs of stercoral matter the next morning when I was called to see him. The patient was very low spirited, believing he was going to die; had very intense pain in the same place as in the previous attacks. Prescribed extract of belladonna, one grain in five pills, to be given one every two hours until relief was experienced; also ordered warm fomentations, bath, and enema.

Aug. 16*th.*—After the first pill was taken, the patient commenced improving; the vomiting and pain ceased; the bowels were moved, and the patient recovered without any other medicines.

Intestinal obstruction is not a common disease, and it is certainly a rare occurrence to see it repeated so many times in a person usually enjoying good health, and having no hernia. What might be the cause? The suddenness of the attacks, and the violence of the symptoms, pointing to the small intestines as the seat of the affection; the absence of any tumor; the entire disappearance of all signs of disease as soon as the obstruction is removed; the improvement following immediately the use of phlebotomy, opium, belledonna, etc., while drastics appear to aggravate the case,—seem to indicate spasmodic contraction as being the foundation of the difficulty. But is it the only cause? Is it not probable that ten years ago, the first time that D—— B—— was affected with this complaint, an organic change, by deposition of plastic matter, took place from inflammation at some point in the small intestine, reducing the size of the canal sufficiently to produce occlusion under certain pathological conditions, but not enough to interfere with the digestive functions in a normal state of the system? Let physicians of more experience than myself decide the question.

M. A. CAMPDORAS, M. D.

Review of Current Medical Literature.

OBSTETRICS AND DISEASES OF WOMEN.

1.—*On Laceration of the Uterus.* By THOMAS RADFORD, M. D., and others, in Obstetrical Society of London. (*Lancet*, New York reprint, October, 1866.)

The author, after briefly alluding to the views of Hunter, Denman, and Douglass on this most dangerous complication to labor, related minutely the histories of nineteen cases which had fallen under his notice. Of this number, in eleven the ages registered were from twenty-one to forty years, and it was found that the accident occurred more frequently between the ages of thirty-nine and forty. The number of labors which each woman had undergone varied from the first to the eleventh, and it was shown that laceration of the uterus happened most frequently in women pregnant for the eighth time; and that in those *enceinte* for the first time, the accident took place quite as often as it did in any of the other cases which were registered. The duration of the labor, from its commencement to the occurrence of laceration (though in some cases not exceeding three or four hours), was generally from ten to thirty hours. Of the various causes or conditions mentioned as producing laceration, slight contraction at the brim of the pelvis appeared to have been the most frequent. The author considered that when the form of the pelvis was only slightly contracted, the os and cervix uteri partially descended during labor into or a little through the aperture of the pelvis, so that as the head of the infant was forced down, the uterine tissues became fixed between this body and the pelvic bones. The fixity of this structure actually formed a *point d'appui* from which the uterine fibres during contraction forcibly pulled; and the greater probability was that sooner or later the tissue either directly tore, or being first contused and softened, yielded. As regards the situation of the laceration, the cervix uteri was the part most frequently affected, and some times with it the body of the organ was also implicated. In eleven cases the laceration was longitudinal, in three transverse, in three oblique, and in one circular. Of the nineteen cases three recoveries took place, or nearly sixteen or seventeen per cent. Dr. Radford, in his concluding remarks, observed that when we contemplated the frequent fatality of laceration of the womb, we were led to inquire whether there were no symptoms which showed themselves as universal precursors of this dreadful catastrophe; and if there were, were we possessed of the means of prevention? In all the cases he now brought before the Society, there could not be found any with premonitory symptoms which *of themselves* would warrant any operative measures being taken in

order to avert the impending danger. Nevertheless, he thought we should carefully consider all the contingent circumstances of protracted labors, and especially of those which were prolonged by mechanical impediments; and whether they were produced by relative disproportion of the capacity of the pelvis to the size of the fœtal head; if so, we should adopt measures of timely delivery.

Dr. Graily Hewitt acknowledged the great value of Dr. Radford's paper, but could not agree in the antiphlogistic treatment mentioned by the author. He (Dr. Hewitt) concurred in the opinion that there was an absence of uniformity of symptoms in these cases, and strongly urged the necessity for early artificial aid in some cases of protracted labor. He related a case of concealed hemorrhage, in which the symptoms were closely allied to those observed in ruptured uterus.

Dr. Playfair could not approve of the treatment which had been adopted in those cases where the fœtus had escaped into the peritoneal cavity. He thought a much better line of treatment would be to perform gastrotomy. He knew of twelve cases in which this operation had been performed, and in some with satisfactory results.

Dr. Braxton Hicks said, that with respect to the symptoms of rupture, it was generally asserted that recession of the head was a constant symptom, but that he had never seen a case where this had taken place. He believed there were many more cases of ruptured uterus than we were cognizant of. Dr. Hicks believed that one of the greatest safe-guards against rupture was the use of chloroform.

Dr. Eastlake observed, that in the diagnosis of rupture of the uterus, some data were furnished by auscultation, the fœtal heart sounds becoming inaudible after the rupture. This point Dr. McClintock strongly insists upon; as also, that in these cases there is very little hemorrhage.

The President (Dr. Barnes) regretted that through indisposition Dr. Radford was unable to be present. He considered the first great cause of rupture was protracted labor, and the object to be had in view was to remove the obstruction as speedily as possible. A second cause was rigidity of the os uteri, and he agreed with the author as to the necessity of incising the os. A third and fourth cause existed in the obliquity of the uterus, which caused it to become jammed in the pelvis; also, when there is a dead fœtus in utero, there is a want of the resiliency which a live child possesses, and the action of the uterus rather tends to squash than to expel it. He also mentioned disease of the uterine tissue as another cause leading to rupture. He thought softening of the tissue might depend upon degeneration, either before labor or during labor, by the pressure of the fœtal head against the pelvis. With respect to gastrotomy, he would say, that Dr. Radford had urged the operation, but that it had been overruled by others. The late President of the Society objected to any operative measures whatever when the fœtus had escaped into the abdominal cavity; and he (Dr. Barnes) had seen a case where it was left, and the woman recovered.

Dr. Brunton observed, that the cases which Dr. Radford had collected were attended by midwives, and he knew that midwives were in the habit of giving very large doses of ergot. He believed that this was one of the great causes of rupture of the uterus; and when it did not cause rupture the placenta was often retained, owing to the irregular contraction of the uterus produced by that drug.

2.—*A Case of Prolapsus Placentæ.* By Dr. P. MUELLER. (*Wuerzburger Med. Zeitschr.* VII., I, 1866, p. 34.)

The author relates a remarkable case of placenta prævia observed in the obstetrical clinic of Wuerzburg. A woman, twenty-seven years old, near the end of her second pregnancy, was suddenly attacked with a profuse genital hæmorrhage, lasting three-quarters of an hour. So far, the course of the pregnancy had been normal. At this time the fundus reached two inches above the umbilicus; distinct ballottement of the head in the left side of the mother; the vaginal portion is shortened; the os not open; no part of the child to be felt presenting; no abnormal pulsation; distinct pains were not perceptible. The diagnosis was: hæmorrhage in consequence of partial detachment of the placenta in the thirty-second or thirty-fourth week of pregnancy, with transverse position of child.

The night following and the next day passed without accident, but in the evening an enormous hæmorrhage again took place, which, within a few minutes, brought the patient upon the verge of death. The os uteri was found opened the size of a florin, the placenta presenting to the right and anteriorly. The vagina was immediately plugged, and ice applied to the abdomen. Patient was extremely anæmic; pulse sixty, scarcely to be felt. Wine and other analeptics were administered.

No essential change occurred until morning, except that the patient recovered strength perceptibly; after midnight the hitherto weak labor pains assumed greater intensity and frequency, and by 5 o'clock expelled the firmly-applied tampon. No further hæmorrhage took place, however. The fœtus, formerly in transverse position, was found vertical, the head in the fundus; the os enlarged to the size of a palm, covered in greater part by the presenting placenta.

While the author prepared to rupture the membranes and rapidly extract the fœtus, *a forcible contraction pressed the presenting placenta into the vagina, and a second contraction forced it through the external genitals, where, with the also prolapsed membranes, it formed a bag larger than a man's head*, which burst by its own weight. The breech was now presenting in the first position, and a third contraction carried it through the pelvis; extraction was accomplished with great facility. The full-grown child was asphyctic; was resuscitated after half an hour's exertions, but died after three hours in a cyanotic condition. The post-mortem showed atelectasis of the lungs, with meconium filling the bronchi to the very smallest ramifications. The mother suffered no further hæmorrhage, and is doing well.

The author takes occasion to remark, that in this case the low position of the fundus, which in the absence of other data led him erroneously to diagnosticate a pregnancy advanced thirty-two or thirty-four weeks only, was occasioned by the development of the placenta in the lower segment of the uterus—an observation already made by the older obstetricians. But the most remarkable feature of the case is the abnormal course of the labor. Parturition can be accomplished in a case of presenting placenta in three ways: Either the birth is interrupted by the death of the mother through hæmorrhage; or, especially in placenta prævia lateralis, the membranes are ruptured in the ordinary way, and the hæmorrhage is arrested by the pressure of the presenting part of the fœtus, or by the awakening of stronger contractions of the uterus; or, lastly, delivery is accomplished by artificial means. In any event, the presenting placenta follows after the previously delivered child. But in this case the placenta was first detached and expelled, and then followed the expulsion of the child.

M. finally urges that the treatment of prolapsus placentæ is *not* identical with that of placenta prævia, where the introduction of the tampon can be recommended. In case of prolapsus placentæ this would be eminently bad practice, since the tampon could not arrest the hæmorrhage, but only prevent the escape of the blood from the genitals. The danger to mother *and* child call for

rapid interference for immediate artificial extraction of the fœtus. Had this course been pursued in the present case, M. thinks he might have saved the life of the child.

3.—*Comparative Merits of Incision and Dilatation of the Mouth of the Womb in Cases of Dysmenorrhœa.* By D. HUMPHREYS STORER, M. D. (*Boston Medical and Surgical Journal*, Sept. 27, 1866.)

[Read before the Boston Society for Medical Improvement, Aug. 27, 1866.]

Every member of this Society must have often been impressed with the tendency which exists in the profession to be unduly influenced, I might, perhaps, with propriety say *overawed*, by the opinion of those who have attained a commanding position in our ranks. This tendency I consider an exceedingly unfortunate one; it destroys self-reliance, individuality; it prevents the physician from faithfully performing his duty; inasmuch as he yields his dearly bought and invaluable experience to the decided, oracular dicta of others. However much we should value and endeavor to profit by the instructions of our fellow-laborers, we should never be willing to relinquish our own convictions, unless satisfied that we are in error; until it is clearly shown that the course we have pursued, and are still pursuing is erroneous. These thoughts have been suggested by the following circumstance: Since our last meeting, a gentlemen called upon me with his wife, who desired my professional advice. She had been an invalid for some length of time, complaining more particularly of dysmenorrhœa. I carefully examined her condition, and found she had a retroflexion of the uterus, the body of the organ being so completely bent upon the commencement of the neck as to cause almost a complete obstruction of the cervical canal—admitting the passage only of a very small metallic dilator. I told the husband what derangement existed, and the course which should be pursued to remove it; that I should advise the introduction of sponge-tents to produce dilatation, and, when this should be accomplished, the wearing of a stem-pessary until the distortion should be permanently overcome. He at once told me that Dr. ——, who had seen his wife, stated that the plan I now suggested would *formerly* have been pursued—that it was not now, however, practised by the profession, but that *incision of the neck* was the only approved method.

As the physician referred to has been a practitioner for quite a number of years, and consequently must have seen a greater or less number of cases similar to the one now spoken of, he evidently in this instance tacitly yielded his opinion to the *weight of authority*.

I think he must have read an article on "Dysmenorrhœa, Metrorrhagia, Ovaritis and Sterility, depending upon a peculiar formation of the Cervix Uteri, and the Treatment by Dilatation or Division," which was published

in the last volume of the *Transactions of the Obstetrical Society of London*, by Robert Barnes, M. D., President of the Society; and also that he must have coincided with the remark of Dr. Marion Sims, made at the meeting at which that communication was presented, "that that Society must be taken as the representative of professional opinion on any subject falling within its domain." Now, however willing we may be to admit the value of the *Transactions* referred to, we are unwilling to allow the infallibility of any, even of the *most distinguished* of that Society. And even at the meeting referred to, it was evident that no little diversity of opinion existed between Drs. Barnes, Baker Brown, Greenhalgh, Routh, Savage, Hewitt, Wyner Williams, and Sims, as to the location of the obstruction in dysmenorrhœa, and the local treatment, whether by dilatation or division.

Thus, Dr. Barnes considers the "seat of the obstruction," to use his own words, "almost invariably at the os externum. The obstruction is due chiefly to the *small, round os itself;* partly to the pointed, elongated form of the lower part of the vaginal portion, and partly to an unusual rigidity of structure of this part which impedes the expanding action natural to the healthy formed os uteri."

Mr. Baker Brown, on the contrary, differed from Dr. Barnes as to the seat of the stricture; he believed it to be in *the cervix itself*, generally accompanied by narrowing, contortions, and reflexion of this canal—the results of inflammation.

Dr. Greenhalgh considered, from a long experience, that in a great majority of cases the stricture exists *at the internal os uteri.*

Dr. Routh coincided with Dr. Greenhalgh.

How utterly absurd to allow our judgment upon *this point* to be swayed by the opinions of either of the gentlemen above quoted, when the experience of every week assures us that the obstruction referred to may, and *does*, exist at any point from the outer to the inner os uteri.

But especial reference I would make as to the manner of *overcoming this obstruction*, wherever it may exist.

Drs. Barnes, Baker Brown, Greenhalgh, and Sims, strongly advocated the employment of the metrotome or hysterotome; that free incision be made; and Dr. Greenhalgh urged that the *internal* os should be dilated as well as the external os.

In other words, after the profession have for a series of years considered that, in the vast majority of cases, a contracted, *an almost impervious os and cervix uteri* may be dilated, and in many instances the suffering produced by this impediment removed by the employment of metallic dilators or sponge-tents, we are told by the President of the Society referred to, that "*incision* is now considered not only as justifiable, but as the *only efficient and permanent remedy* for dysmenorrhœa." Mr. Baker Brown, Drs. Greenhalgh, Routh, and Sims appear to have coincided with this view of the subject.

And why is this plan so strongly advocated? Dr. Barnes says: "Hæmorrhage, pyæmia, cellulitis, peritonitis, have undoubtedly followed dilatation;

and it is certain that in many cases, however good the dilatation effected by bougies or tents may appear at first, it is *not of long duration*. I suppose there is no dilatation by instruments more powerful than that effected by pregnancy and labor, yet after giving passage to a full-grown child, the peculiar cervix will some times completely resume its old vicious form."

Mr. Baker Brown agreed with Dr. Barnes "that *dilatation* was an *inefficient* and only temporary remedy for dysmenorrhœa arising from the stricture of the canal."

Dr. Routh "had seen *cellular abscess* and *death* follow the use of sponge-tents."

We remember having seen in some New York journal, a year or two since, similar remarks to have been made by Dr. Fordyce Barker, and others, respecting the employment of *sponge-tents;* that they had seen injurious results produced by their employment. It would be presumptuous in the extreme for me to doubt the statements of these gentlemen; I believe they stated the truth; I allow all they utter *may* occur. But is any known remedy *always* reliable? Is any known operation always successful? Is not an invalid some times *made the sicker* by the dose administered? the suffering one made *permanently* a sufferer by the surgeon's knife? May not some of the evils thus produced by sponge-tents be unnecessary? May not the time at which they are introduced, the size of the tent, the manner of its introduction, influence the effects produced? Not unfrequently, particularly in hospitals, this operation would be advised by the attending physician, but be performed by a less skillful hand, even by a nurse. Should there be an unusual excitement of the parts, such as frequently exists just preceding or following a menstrual period, it would of course be contra-indicated. The size of the the tent is of great importance. We can readily conceive that a large tent, which is capable of being dilated to a great extent, should cause much distress at the moment of introduction, and produce long-continued and serious constitutional derangement. The operation itself may be improperly performed. If, instead of being carefully introduced, and the effects produced being watched, the dilator is carelessly, roughly, unfeelingly forced into the sensitive parts, suffering to a greater or less extent must inevitably be produced. This is self-evident. From a somewhat extensive employment of sponge-tents during the ten past years, for the treatment of dysmenorrhœa and sterility, I have formed conclusions different from those of the gentlemen of whom I have spoken. I have not unfrequently been disappointed in the result hoped for. The local obstruction has almost always been overcome by the long-continued, persevering employment of the dilator; but the opened canal does not always remove the condition thought to depend upon its closure—dysmenorrhœa and sterility still remain. I have, however, never seen the ill effects spoken of from the employment of tents. I can not recall a single instance where more than a few hours inconvenience has been produced; and in such cases the expanded sponge, when removed, has proved to have been originally much larger than it was supposed to be—showing that he who employs

these tents should be acquainted with their uncompressed dimensions. My experience has taught me, then, that these contractions, however firm they may be, may almost invariably be overcome. The physician need not feel that the part is undilatable because the application of three, or five, or half a dozen tents does not overcome it; in a case occurring in my practice about a year since, *eighteen* sponge tents were introduced at intervals of two and three days before the canal was opened. My perseverence was rewarded by the perfect relief of the patient. I could point, were it necessary, to several cases where, after years of sterility, the sufferer has been relieved and borne children, and in the intervals of their childbearing have suffered no dysmenorrhœa. I have repeatedly seen cases of dysmenorrhœa remain relieved for years, and known no return. In a word, I have relied upon dilatation to relieve these affections, and whatever opinions may be advanced by others, so long as I feel we have a remedy from which we can confidently expect relief, and very rarely observe any injurious effects, I shall feel it my duty to employ it.

That cases do occur where the difficulty *can not* be removed by dilatation, there can be no question; but "that incision is the only efficient and permanent remedy (in most cases) for dysmenorrhœa," I unhesitatingly deny.

Let us for a moment look at the method proposed. Those who advocate it should of course be satisfied that it has superior claims over the means now employed. I have thought the ill effects produced by *distension* might be occasioned by want of care; but those arising from incision *may* follow the operation of the most skillful surgeon who advises it, when the metrotome cuts through the walls of the *inner os;* and Dr. Barnes states, to employ his own language, "there is no doubt that the surgeon has actually cut through the substance of the uterus, and wounded the plexus of vessels outside; hence severe and dangerous hæmorrhage has ensued, and inflammation of the peri-uterine tissues." And even supposing the operation should be successfully performed, it is acknowledged by Dr. Routh, one of its advocates, that such an amount of contraction frequently exists as to render it necessary to have a dilating substance worn for a considerable length of time to prevent its perfect occlusion"; and Dr. Williams observes that often times no relief is afforded. He had seen a patient whose cervix uteri had been slit up on both sides, forming two large protruding lips, without affording any relief to the sufferer." Where the external os has been almost cartilaginous to the feel, I have overcome the obstruction with the hysterotome; but I have never attempted to divide the internal os. I can not, however, recall the instance where it was required.

Fortunately for those who object to *unnecessarily experimenting* upon the os and cervix uteri, there were those at the meeting when Dr. Barnes read his paper whose opinions coincide with ours upon this subject. Thus, Dr. Savage, physician to the Samaritan Hospital for Women, who was in the habit of treating the severest cases of the character I have spoken of every week, assures us he never failed to remove the obstruction with the sponge-

tent; and Dr. Grally Hewitt observed that where the cervix uteri was not hard and tense, he preferred to employ the tents as dilators. With these opinions Dr. Williams also coincided.

Enough has been said, I trust, to prove that the profession *generally* do not advocate the *indiscriminate incision* of the cervix uteri in cases of dysmenorrhœa; that the physician should yield his scientific opinion only when convinced of his error; that carefully attested facts are of infinitely more value than the dogmatic teachings of the *highest authority*.

MATERIA MEDICA AND THERAPEUTICS—TOXICOLOGY.

1.—*Hypodermic Injections of Quinia.* By R. B. MAURY, M. D., Port Gibson, Miss. (*American Journal Medical Sciences*, October, 1866.)

The author experimented on this subject when Surgeon C. S. A., in charge of General Hospital, Greenville, Alabama, and quotes as follows from his report to the Surgeon-General, at Richmond:

This report embraces the history of twenty-five cases of malarial fevers, of which six were bilious remittent, five were quotidian intermittent, thirteen were tertian intermittent, and one a case of double tertian intermittent. The instrument used was the ordinary Wood's syringe for hypodermic injections, carefully graduated and found to carry thirty minims. The seat of its introduction, in the treatment of these cases, was usually the cellular tissues of the arm, about the insertion of the deltoid; sometimes it was introduced into the forearm. The preparation used was a perfectly clear solution of the sulph. of quinia in water with a sufficient quantity of dilute sulphuric acid; sixty minims of the solution contained eight grains of quinia. In cold weather the solution required to be well warmed before injecting it, because at moderately low temperature it crystallizes rapidly and obstructs the syringe; it is then also painful to the patient.

In most of the cases, six grains was the entire quantity of quinia used during an intermission or remission; in the severer cases eight grains was the quantity used. Cinchonism was fully produced in from forty to sixty minutes.

The cases reported were from the troops on duty at Spanish Fort and the other defences of Mobile; they had all suffered more or less severely from malarial disease during the previous summer and fall; therefore, as it would have been necessary to repeat the administration of quinia, if given by the mouth after the paroxyms were broken; so in this mode of treatment, it was generally repeated twice by injection, to prevent a return of the paroxysm.

On two occasions, it will be seen that the chill occurred, in spite of the remedy, at its usual hour, or at some irregular hour on the same day, as it often does after the ordinary mode of administration. In the intermittent cases, no patient was injected until at least one chill had occurred after his admission to hospital; in the cases of bilious remittent, no quinia was used at all until the the third or fourth day of the disease.

It is thought that the hypodermic may be preferable to the ordinary mode of administration under the following circumstances:—

1st. Where the economy of quinia becomes an object of great importance; this method requiring about one-third the quantity necessary if given by the mouth.

2d. In cases of fever, in which, from excessively irritable stomach, it is difficult to use remedies in the ordinary way.

3d. In the congestive forms of fever where absorption by the stomach is slow; or, in cases where there is not sufficient time to get the patient under the influence of quinia, given in the ordinary way, before the expected hour of return of a congestive paroxysm, life might be saved by introducing it through the cellular tissue.

The occasional occurrence of abscess is the only objection noticed to the hypodermic method; this occurred in two instances out of the twenty-five cases reported, and was attributed to a repetiton of the injection in the same place; subsequently, when the injection was repeated, it was put into the forearm.

It may be observed from the above report that so far as the constitutional effects of the medicine are concerned, I claimed no more for it than is accomplished in the administration of quinia by the mouth, and I am not inclined to believe, with Dr. Chasseaud, of Smyrna, that relapses will be less frequent after this than after other modes of treatment.

2.—*The Effect of Large Doses of Hyposulphite of Soda.* By Dr. G. Polli. (*Med.-chir. Rundschau*; from *Annali di Chim. appl. Milano, Aprile*, 1866.)

Polli experimented with the largest doses of sulphites the human organism could bear without injury, in order to determine the changes to which the salt is subjected within the body, when and in what state it appears in the urine, and what physiological functions are affected by it. The result is as follows:

a. A large dose of the salt (*hyposulphite of soda*) enters the blood and reappears in the urine as sulphates, in small proportion also, as sulphites and hyposulphites. But these salts diminish after the administration of the remedy is stopped, more and more, so that after twenty-four hours no trace of them is found.

b. The ingestion of hyposulphites diminishes the excretion of phosphates. (? Requiring further proof.)

c. The presence of sulphites and hyposulphites in the urine, and perhaps an alteration of some of its organic constituents effected by them, is capable of retarding its ammoniacal decomposition. This property, therefore, justifies their employment in catarrhal cystitis, and in all affections of the uropoetic organs, where the formation and retention of purulent matters cause injurious accumulations in the urinary apparatus.

d. The sensation of intense cold, of shivering, of stiffening of the extremities and muscular debility, are easily explained by the *reducing* (deoxydizing—REV.) effect of the hyposulphite. The latter, in its passage through the system, is almost entirely transformed into the sulphate and bisulphate of soda.

And calculation really shows that 100 grains hyposulphite of soda require 12.90 grains or 8.977 litres oxygen, to be converted into the sulphate of soda. . . . . The abstraction, however, from the human organism of 9 litres of oxygen in the space of twenty-four hours, certainly can not take place without causing a diminution of temperature and of the mobility of the muscles. Another consequence of it is a perceptible diminution of thirst.

A salt, P. concludes, which can be introduced into the body in large doses (five grains were given every hour) without particular discomfort, which occasions no considerable disturbance in the primæ viæ, which effects frequent and fluid, painless evacuations, diminishes the production of heat and the general oxydation, might properly be regarded as an excellent antiphlogistic in the true sense of the word.

8.—*Chloride of Sodium in the Treatment of Wounds.* (*Druggists' Circular*, October, 1866; from *Union Méd.*)

This is one of the most important discoveries of the present day for inducing the speedy cicatrization of suppurating wounds, and for obviating the dangers that sometimes result. A great many agents have been employed for this purpose; as coal-tar, phenic acid, camphorated alcohol, chlorate of potassa, and other compounds of chlorine, and latterly the sulphites. Among these different agents, none are more useful as disinfectants

than the compounds of chlorine; but, strange to say, the chloride of sodium, which is the most common, and is always at hand, is rarely used by the profession. It is not, however, that experience has failed to testify to its worth; for many very able and interesting articles have been written on its use in the treatment of wounds, and submitted to the profession. Latterly Dr. V. Dervandre has published an article on the value of the chloride of sodium in the treatment of wounds, the more valuable, adds this author, because it can be always procured. The first effect of chloride of sodium on a wound which is fetid is to induce the immediate disappearance of the bad odor. Another immediate phenomenon observed is the pinkish hue which it gives the decomposed sanguineous blackish liquid which covers the wound. At the same time, there is experienced a sensation of cold and of pricking in the wound, which may even become slightly painful. The suppuration diminishes rapidly in quantity, and if sanious it becomes healthy in a few days. The wound granulates and cicatrizes rapidly. The change evidenced in wounds by the chloride of sodium has a happy effect on the system. The appetite improves, and the patients acquire strength. In support of the value of a solution of this salt, Dr. Dervandre reports four hundred cases of wounds thus treated. In one case only was there pyæmia. There was neither erysipelas, nor tetanus, nor hospital gangrene present in any of his cases, though the hygienic condition of the hospital under his care was bad. The solutions used by Dr. Dervandre are not of the same strength. At first he uses a solution of about two drachms to two pints of water. In a few days afterward he resorts to concentrated solution. These solutions are injected in fistulous tracts, or on the surface, according to the nature of the wounds.

4. *Use and Abuse of Poultices.* By Dr. RICHARDSON. (*American Journal of Medical Sciences*, Oct., 1866; from *British Medical Journal.*)

In his lectures recently delivered at the College of Physicians, Dr. Richardson made the following remarks on the subject of poultices:

The application of moist heat in the form of poultice to suppurating parts requires, I think, remodeling, in order that it may be placed on a true scientific basis. I am afraid that the common recommendation, "You must put on a poultice," is too often among us all an easy way of doing something about which we are not quite sure, and concerning which it were too much trouble to think long. From what I have recently observed, I fear that mischief is often done by a poultice, which might well be avoided. The people have always a view, that a poultice is applied to "draw," as they say —a term in truth which, though very unsophisticated, is in a sense a good term, for it means what it says. The question for us is, whether it be sound practice to carry out as a general rule the "drawing" process, either by fomentation or by poultice.

When a part is disposed to suppurate, the first step in the series of changes is an increased flow of blood through the capillary surface, followed by obstruction, and thereupon by an excess of sensible heat derived from the friction that is set up. Then follows transudation of liquor sanguinis into the connective tissue, and its transformation, under the influence of heat, into what is called purulent fluid. When to the part in this state we apply moist heat, we quicken suppuration, mainly by upholding the temperature; at the same time, we secure the transference of water from the moist surface into the fluids of the inflamed part, by which tension of tissues is produced, and in the end yielding of tissue at the weakest point.

When the suppurating surface is circumscribed, the rapid induction of the process may be attended with little injury; but when the surface is large, and when the exuded fluid is thrown into loose structures where it can burrow readily, the practice, I think, can not be good to extend the mischief. Hence, in the treatment of carbuncle and phlegmonous erysipelas, it can not, I opine, be sound practice in the early stage to apply moist heat. Experience also, not less than principle, warrants this conclusion. In cases of carbuncle especially, I have of late altogether avoided the application of moist heat in the early stages; and, I feel assured, with good results.

But when, in the course of local disease, suppuration is actively established, and is naturally circumscribed; when the increased temperature of the part has fallen to or below the natural temperature—then the value of moist heat comes on with full force; then the tension which is exerted determines the escape of fluid at the weakest point of the surrounding tissue, and, when the fluid escapes or is liberated by the knife, the escape for a long period is aided by the application of moist heat.

The continued application of moist heat for a long time after the escape of purulent fluid is again, I conceive, indifferent practice. It sustains discharge; it sets up unhealthy decomposition of fluids; it produces a thickened, soddened condition of skin, most favorable to the production of sinus; and it retards recovery. When a surface is freely open, and suppurating, dry and not moist heat is the remedy. We are in want in these cases of a simple invention; we require something which we can apply as readily as a poultice, which shall keep up the temperature of the part, and at the same time take up moisture, and gently desiccate, without injuring the tissues.

5.—*The Treatment of Poisoning by Phosphorus.* By Prof. H. V. BAMBERGER. (*Wuerzb. Med. Ztschr., VII.*, p. 57, 1866.)

B. thinks the remedies hitherto in use in cases of phosphoric poisoning are so little reliable as to justify searching for others. The chief problem seems to be to find a remedy which prevents as far as possible the vaporisation of the phosphorus, because decidedly the vapor of phosphorus alone is the injurious agent.

Now the author observes, that if pieces of phosphorus are placed in a test-tube with some water, and a little solution of sulphate of copper is added, the phosphorus at once assumes a black color, and after a short time a coating of metallic copper is formed upon it; all the sulphate is thus finally reduced, so that no copper can be detected in the solution. In this state the formation of vapor is very trifling; the phosphorus has none of its usual odor, and is not luminous.

The same chemical process no doubt takes place in the stomach also, but a sufficient quantity of sulphate of copper would cause vomiting as soon as introduced. B., therefore, experimented with other salts of copper, but found the carbonate alone suitable to the purpose. This can be administered, on account of its insolubility in water, in larger doses, without causing emesis, and acts rapidly if a small quantity of acetic acid is added; the acetate being reduced by the phosphorus immediately after it is formed.

BAMBERGER, therefore, recommends the following treatment in a case of poisoning by phosphorus: first, an emetic,—and as such, sulphate of copper alone is to be used. After repeated vomiting, a weaker solution of the same remedy is to be continued. If the patient can bear this without vomiting immediately and often, so much the better; if not, carbonate of copper (oxyde) is given, in doses of from four to eight grains, suspended in water; at first frequently repeated, several doses in a half hour, each dose followed by a drink of one half to one spoonful of vinegar in some water. In the intervals, it might be well to counteract the beginning inflammatory affection of the stomach by cold drinks, or small pieces of ice. After a few hours, he considers it proper again to administer sulphate of copper as an emetic to remove the remaining pieces of phosphorus now coated with metal, which no longer adhere so tenaciously to the mucous membrane. The author is the more disposed to urge the adoption of this treatment in a case of this kind, because he feels certain that nothing is lost by neglecting the methods of treatment hitherto in vogue.

Scientific Department.

METEOROLOGY OF SEPTEMBER AND OCTOBER, 1866.

BY GEORGE ENGELMANN, M. D., ST. LOUIS.

	SEPTEMBER.		OCTOBER.	
	1866.	Average.	1866.	Average.
Barometrical Pressure—				
Mean	29″.526	29″.559	29″.612	29″.574
Highest	29″.804	30″.214	29″.985	30″.352
date	26th.	29th, 1857.	16th.	15th, 1856.
Lowest	29″.206	29″.020	29″.040	28″.910
date	1st.	30th, 1843.	21st.	3d, 1854.
Temperature—				
Mean	68°.3	69°.4	57°.7	55°.8
Highest	87°.5	102°.5	79°.5	89°.5
date	2d.	2d, 1864.	20th.	2d, 1858.
Lowest	39°.5	35°.0	30°.5	21°.0
date	21st.	30th, 1839.	31st.	31st, 1863.
Evaporation	4°.1	6°.2	5°.5	5°.4
Relative Humidity	78.2	70.9	68.6	67.9
Rain—				
Quantity	10″.53	3″.25	2″.01	3″.39
Days on which it fell	17	7	8	7
Thunder-storms	4	4	1	2
Principal Winds	NW. next SE.	SE.	SE.	SE. next W.
Cloudiness	5.7	3.8	3.6	4.0
Fair days	8	15	17	15
Variable days	16	13	12	12
Days without sunshine	6	2	2	4
Stage of River—				
Mean	14′.1	9′.5	11′.5	8′.7
Extremes	11.0′—21′.0	3′.0—21′.0	7′.7—19′.9	2′.8—19′9

Both months, September and October, exhibited very unusual, and, to a great extent, antagonistic meteorological conditions. September had a lower stage of the barometer than usual; a much lower temperature; a lower degree of evaporation; fewer fair days; a much greater quantity of rain, rainy days, and relative humidity; and a greater number of days without any sunshine. October was in most or (with the exception of relative

humidity) all these conditions the exact reverse of September. In thirty-two years I have not observed so cool and cloudy a September, nor so wet a one. Our October is some times warmer, some times fairer, and even drier, but it rarely contrasts more agreeably with so unpleasant a preceding month.

But, disagreeable though this September was, it had a most favorable effect on the health of the city, and especially of the country. I do not mean hereby the cessation of epidemic cholera which we witnessed during September, for the weather had very little to do with it; so little, that while during this month it diminished and died out here in St. Louis, it commenced or increased in other Western cities which were subject to the very same meteorological influences. What I intend to credit September with is, that its coolness, and especially its humidity, prevented, to a great extent, the development of malarious diseases. In October they appeared in a very moderate degree, not rarely in the form of intermittent neuralgia. Cases of dysentery were also observed, and that disease of our late fall and early winter months, typhoid fever, began to show itself; generally, however, in a mild form. No epidemics of any kind, no exanthematic fevers have been observed, and with the end of the month the sanitary condition of the city is unusually favorable, and promises to continue so.

SCIENTIFIC DEPARTMENT.

ATMOSPHERIC ELECTRICITY, TEMPERATURE, AND HUMIDITY.

Observations made at St. Louis, Missouri, based on daily observations at 6 9, 12, 8, 6, and 9 o'clock, from morning till night.

BY A. WISLIZENUS, M. D., ST. LOUIS.

1.—*Monthly Mean of Positive Atmospheric Electricity.*

SEPTEMBER.

YEAR.	6 A. M.	9 A. M.	12 M.	3 P. M.	6 P. M.	9 P. M.	MEAN OF MONTH	NO. OF THUNDER STORMS.	PREVAILING WINDS.
1861...	2.1	2.9	3.9	3.4	4.2	1.7	3.0	3	SE. & NW.
1862...	3.2	3.2	3.4	3.1	3.0	2.0	3.0	6	SE.
1863...	6.3	4.6	4.9	4.0	6.5	2.6	4.8	4	SE. & S.
1864...	3.2	1.9	2.0	1.2	1.4	1.4	1.8	4	SE.
1865...	0.7	0.7	1.8	1.3	2.4	0.5	1.2	4	SE.
1866...	2.0	2.9	2.6	3.5	4.2	2.2	3.2	4	SE.
Mean.	2.9	2.7	3.1	2.7	3.6	1.7	2.8	4	.....

OCTOBER.

1861...	7.0	9.7	7.2	5.5	7.3	6.0	7.1	1	SE. & S.
1862...	9.0	8.4	7.7	5.5	8.7	7.2	7.7	1	NW. & SE.
1863...	10.9	15.0	13.2	9.3	13.8	12.4	12.5	3	SW. & NW.
1864...	5.9	7.6	5.2	3.2	6.7	3.9	5.4	1	W. & SW.
1865...	4.9	7.3	7.2	4.2	5.8	2.5	5.3	0	SE.
1866...	5.9	8.0	8.7	6.9	8.0	4.3	7.0	2	SE.
Mean.	7.3	9.3	8.2	5.8	8.4	6.1	7.5	1.3	.....

2.—Monthly Mean of Temperature, Fahrenheit.

SEPTEMBER.

YEAR.	6 A. M.	9 A. M.	12 M.	3 P. M.	6 P. M.	9 P. M.	MEAN OF MONTH.
1861	60.9	67.8	75.8	75.8	70.5	64.9	69.1
1862	62.5	70.0	79.0	79.1	73.1	68.9	72.1
1863	58.2	66.5	76.0	77.4	71.8	65.3	69.2
1864	62.5	70.4	79.2	81.5	75.2	68.7	72.9
1865	72.8	76.1	83.5	83.5	77.8	73.5	77.8
1866	57.5	62.6	67.4	68.8	65.7	62.0	64.0
Mean	62.8	68.9	76.7	80.9	72.8	67.2	70.8

OCTOBER.

1861	49.1	55.0	62.9	67.5	58.5	54.5	57.6
1862	49.0	55.8	62.9	64.1	58.4	53.5	57.3
1863	41.3	45.1	51.8	53.8	49.9	45.8	48.0
1864	45.1	50.2	58.8	59.7	54.5	50.2	53.1
1865	50.8	56.5	64.6	64.9	59.8	56.4	58.8
1866	51.1	56.5	64.5	66.6	60.9	56.1	59.3
Mean	47.7	53.2	60.9	62.8	57.0	52.7	55.7

3.—Monthly Mean of Relative Humidity.

SEPTEMBER.

YEAR.	6 A. M.	9 A. M.	12 M.	3 P. M.	6 P. M.	9 P. M.	MEAN OF MONTH.
1861	93.1	78.4	66.2	65.2	74.7	86.5	77.3
1862	92.3	75.7	59.1	59.0	74.0	85.0	74.2
1863	88.1	70.7	55.9	53.1	64.6	77.1	68.2
1864	85.3	68.9	52.0	46.9	58.4	73.0	64.1
1865	87.2	79.0	64.5	64.5	78.8	86.7	76.8
1866	94.5	83.4	72.8	71.0	80.2	89.2	81.8
Mean	90.1	76.0	61.7	60.0	71.8	82.9	73.8

OCTOBER.

1861	92.7	82.1	65.2	62.5	74.0	82.9	76.6
1862	86.5	68.6	53.0	53.2	65.5	76.7	67.2
1863	90.3	78.5	65.7	60.5	70.4	80.8	74.4
1864	86.4	78.2	56.5	54.0	66.0	71.3	67.9
1865	90.0	75.2	60.7	62.2	74.9	81.5	74.1
1866	88.4	77.6	60.7	54.7	69.1	79.9	71.7
Mean	89.0	75.9	60.8	57.8	70.0	78.8	72.0

Editorial Department.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

LECTURES ON EPILEPSY, PAIN, PARALYSIS, AND CERTAIN OTHER DISORDERS OF THE NERVOUS SYSTEM. By CHARLES BLAND RADCLIFFE, M. D., F. R. C. L., etc. Philadelphia: Lindsay & Blakiston.

The physiology of the nervous system has long been regarded as one of the most worthy, as it is unquestionably the most difficult study which has engaged the attention of medical minds. The brightest stars in the profession have spent their energies, and exhausted all known resources, with but little avail in elucidating the mysteries which enshroud nervous action. Galvani, Nobili, Flourens, Prochaska, Magendie, Longet, Volkman, Matteucci, Marshall Hall, Du Bois-Reymond, Brown-Séquard, Bernard, with many others, have devoted their talents to this field of study; and although from each some point has been gained, or some fact established, yet at this day we must be frank enough to acknowledge, that no theory has been advanced supported by such an array of facts as to impress us with its truth. The appearance of a new theory, then, should at least excite our curiosity, and invite us to examine into its merits and demerits, and judge ourselves of the import of the facts presented, the weight which the observations and experiments should exercise, and the truth of the reasoning on this most important subject.

The work before us contains more than its modest title would lead us to expect; for, instead of being simply a guide in the treatment of "epilepsy, pain, and certain other disorders of the nervous system,"—as one would suspect,—the greater portion of it is filled with an ingenious dissertation upon the physiology of muscular and nervous action, treated in an interesting and concise manner, and leading the reader by easy grades of logical deduction from the premises with which he begins, to the theory and

treatment with which he concludes. It is emphatically an original work, and this of itself should draw from us an attentive consideration, especially as the ideas are not promulgated through a love of novelty, but with the earnest conviction that they are true, and if adopted, would materially and advantageously influence the treatment of many diseased conditions. The opinions of the author are the result of long study and close observation, and though new to the majority of readers, have been gradually taking form and substance in his mind for years past. Dr. Radcliffe, some twelve or more years ago, published the first of his thoughts on the subject. They were then crude and ill-digested; the subject as he then presented it, to give his own opinion now, "was only a confused mass of thoughts, tumbling over each other in the dark, when the fancy was yet in its first work, moving the sleeping images of things toward the light." But in his last publication his opinions are perfected by more matured thought, substantiated by facts drawn from the rich mines of observation and experiment by such men as Matteucci, Brown-Sèquard, and Du Bois-Reymond, and tested to some extent, at least, by bed-side experience. Dr. Radcliffe does not advance a perfect theory; does not give a complete explanation of nervous action; as even supposing his theory true, there is much, very much in minutiæ to be worked out by patient labor, especially at the bed-side. But he goes further than any one before him toward a fusion of the facts known for years past, and those lately demonstrated, into a theory very ingenious, and more complete than any yet advanced. To accomplish this he has started out on a new path; he views the subject from a new stand-point; and as this is established *de novo*, and on what seems a firm basis, his opinions are more worthy our serious consideration.

Since the renowned observations of Galvani upon the frog's leg, by which his name will be perpetuated to future generations, physiologists have differed in their belief as to the existence of a peculiar electricity, termed *animal electricity*. At one time, following the lead of some celebrated observer, the existence of

it has been denied; yet at another, influenced by some new observation, the weight of opinion would veer to the contrary. Many experiments would demonstrate an electricity following the known laws which govern ordinary electricity; whilst others, though not contraindicating the existence of a fluid, would throw doubts upon its identity with common electricity. The existence of animal electricity, the author believes, has been unequivocally demonstrated, and it is upon this fact that his theories and opinions are based. After studying the observations and experiments of others as well as his own; after familiarizing himself with the laws of animal electricity, as deduced from a great variety of ingeniously conducted, and delicate experiments; he educes a theory which he gives to the world as an embodiment of his views, and which, if adopted, will necessitate an entire change in the treatment of convulsions, pain, and some varieties of paralysis. "The joint labors," he says, "of M. M. Matteucci and Du Bois-Reymond have left no room for entertaining any doubt as to the reality of animal electricity. This will appear sufficiently in the sequel, where many of the experiments which furnish the demonstration, will have to be referred to particularly. In the meantime it may be said, that M. Matteucci has demonstrated, in the most unequivocal manner, that animal electricity is capable of decomposing iodide of potassium, and of giving 'signes de tension avec un condensateur délicat,' as well as of producing movement in the needle of the galvanometer; and not only so, but also—a fact, the discovery of which will always give M. Matteucci a place in the very foremost rank of physiological discoverers—that muscular contraction is accompanied by an electrical discharge analogous to that of the torpedo. And, as for M. Du Bois-Reymond, it may be said that he has demonstrated that there are electrical currents in nerve, in brain, spinal marrow, and other great nervous centres—in sensory, motor, and mixed nerves—in the minutest fragment, as well as in masses of considerable size; that the electrical current of muscle which had been already discovered by M. Matteucci, may be traced from an entire muscle to the single primitive fasciculus; that Nobili's

'frog current,' instead of being peculiar to the frog, is nothing more than the outflowing of the currents from the muscles and nerves; that the law of the current of the muscle in the frog is the same as that of the current of the muscles in man, rabbits, guinea-pigs, and mice, pigeons, sparrows, etc., etc.,—in creatures belonging to every department of the animal kingdom; that the law of the current in muscles agrees in every particular with the law of the current in nerves, and also with that of the feeble currents which are met with in tendon and other living tissues; and that there are sundry changes in the current of muscle and nerve, under certain circumstances—as during muscular contraction during nervous action—under the influence of continuous and interrupted galvanic currents, and so on, which changes, as I shall hope to show in the sequel, are of fundamental importance in clearing up much that would otherwise be impenetrable darkness in the physiology of muscular action and sensation."

He endeavors to show that the natural electricity of muscle and nerve, during the state of rest, is in a statical, and not in a current condition,—a condition which is no other than that of tension; that the electrical discharge, analogous to that of the torpedo, which M. Matteucci has shown to accompany muscular action, is essential to the interpretation of muscular action; that it is necessary to borrow light from the discoveries of M. Matteucci in natural electricity, as well as from those of Du Bois-Reymond, in order to obtain a clear insight into the physiology of sensation, and the pathology and therapeutics of pain. In accomplishing these endeavors he has accumulated an immense amount of fact and observation, interesting in the extreme; some old, but much new and startling; and has so interwoven and presented it that his deductions are logically true, and can not but wield considerable influence over the minds of his readers. With many of his deductions the mass of his readers will acquiesce without hesitation, but there are others which only those who partake of his enthusiasm on the subject can adopt. The theory seems to be a purely physical one. "Irritability," "tonicity," "nervous influence" (apart from electricity), he

looks upon as indefinite terms, explaining nothing, and hence unworthy to be used. Purely physical as it is, it should not be discarded by any one on that account alone; for he says very truly, "that a theory, based upon the well-known laws of physics, may have some claims to be regarded as supplying a satisfactory explanation; but that a theory, which assumes as its fundamental principle, that the mystery of life is a sufficient explanation for every difficulty, can have no such claims. For what explanation is it to say that a nerve produces contraction in muscle, or that a muscle contracts because muscle and nerve are endowed with a living capability of doing so?"

It would be unsatisfactory, both to the reader and to ourselves, to follow the author through his method of treating the subject. The book must be read to be properly appreciated, and read attentively. His illustrations and experiments are so numerous, and have such a bearing on the subject matter, that it would be difficult to cite a sufficient number to make his meaning clear, without extending this notice much beyond the limits intended. It will suffice to say in regard to his therapeutics, that they are based on his physiology; deduced from the laws which he has shown control muscular and nervous action. He advises, among other things, as to diet, a greater abundance of oily matter, and less of lean meat; plenty of exercise; the avoidance of purgation; the use of cod-liver oil; phosphorus, in the form of hypophosphites, and bromide of potassium; opium in certain cases may be used, as also blood-letting, to prevent certain consequences of convulsions. Alcoholic stimulants, he believes, are very trustworthy antispasmodics in the prevention and treatment of convulsions; indeed, his whole therapeutics are based on the idea, that the vital power is to be reinforced; not the contrary doctrine. This treatment, in particular, refers to chronic cases. Whilst we can not coincide entirely with his views of treatment, yet the experience of very many physicians, who do not adopt his opinions of muscular and nervous action, will corroborate his views as to the use of stimulants and good diet in many cases of convulsions, in children especially. The exhibition of ammonia, brandy, and

good diet, has, we are convinced, saved many lives which would undoubtedly have been lost by a contrary practice. His treatment of pain of a neuralgic character, tremor, etc., is based on the same view, and partakes of a like character.

The book is published by Lindsay & Blakiston; is illustrated with many engravings, which serve as great aids in understanding the text; and is printed with large, plain, beautiful type. To all those who are not content with the crude ideas of older days; who are not willing to accept irritability, tonicity, and such like terms as sufficiently comprehensive; who desire to partake of the advantages which modern science has infused into medicine,—we would cordially recommend this work: and if they can not adopt all the views of the author, we are sure that they will, at least, be much interested in the mass of facts which he has brought together with so much ingenuity and industry. Modern progress in medicine is still reviled by some, but most unjustly. Medicine can not but be influenced by the discoveries daily made, and many of the still clinging mysteries be swept away; and though some enthusiasts, not prompted by a true love of science, but more by a selfish desire of bringing their own views before the world, may bring discredit on the term progress, yet reasonable, intelligent men should winnow the wheat from the chaff, and garner up for future use. Progress has been remorseless in tearing away the ignorance which has so long enshrouded the fair form of animal life. Day by day the dark mantle has given way before patient observation by master minds, and there stands revealed more and more of that beautiful structure which is but a part of a grand whole,—a part of that which shows itself in the humble algæ, and lichen, and in the stately oak,—in the microscopic infusoriæ, and mighty, intelligent man. What a space seemingly separates one from the other, and yet how beautifully are they interwoven together! How different in form and powers! And yet by the magic of organization, by the power of those laws which progressive science has made known to us, they are made one and indivisible. Even in the nervous system of man, which has its centre in that "dome of thought, the palace of the soul," and

invested for that reason with the mystery which surrounds man's other self, science is busy laying open the labyrinth, and presenting it to us divested of much of its mystery. We should never shrink from the investigation of any subject, because it may sweep from us the cobwebs which long familiarity has induced us to hold sacred, but labor on, feeling sure that at some day all will be made plain; and then we shall have presented to us complete, the beautiful laws which rule the universe, and which alone could emanate from a Great First Cause. C.

INSTRUCTIONS IN THE PREPARATION, ADMINISTRATION, AND PROPERTIES OF NITROUS OXIDE. By GEORGE T. BARKER, D. D. S., Professor, etc., etc. Philadelphia, 1866. 8vo. pp. 61.

The above work is written especially for the use of dental surgeons; yet its contents may interest the general practitioner as well, for the practicability of adopting the nitrous oxide as an anæsthetic in surgical operations of short duration, at least in hospital practice, seems to us to be well established, and will be greatly enhanced if, as our author promises, Prof. Vander Weyde carries out his intention of preparing the agent in a liquid form (by compression), and supplying it to the profession, much like soda-water is supplied, in strong metal cylinders.

The treatise gives full "instruction" on the nature and chemical properties of laughing gas, directions for its preparation, descriptions of the best apparatus for the purpose, and, most necessary of all, explicit rules for the safe administration of the gas. Dr. Barker especially urges dentists to abolish the vile practice of allowing the expired air to pass back into the gas-bag, thus mixing the nitrous oxide, intended for respiration, with carbonic acid. New mouth-pieces, constructed for the purpose of obviating this, are described and figured.

The sections most interesting to the medical reader, on the physiological properties of nitrous oxide, and on its contra-indications, are rather deficient and superficial. We are told that "nitrous oxide, like alcohol, acts as a stimulant"; that it, "by its stimulating effect, increases nerve force, and brings vital

action above its standard," in contradistinction from ether and chloroform, which "probably act as sedatives by depressing the nervous system," etc. *The effect of this increase of nerve force is "total insensibility."* We refrain from entering into a critical examination of this theory, but can not help expressing our opinion that it is rather too bold and unusual to stand unsupported, as it does in the pages before us, by something like convincing argument or experimental proof.

Dr. Barker brings evidence of the adaptability of the new anæsthetic to surgical practice, and thinks that in future "new uses will be made of this agent, as valuable perhaps as its anæsthetic properties." G. B.

THE SCIENCE AND PRACTICE OF MEDICINE. By WILLIAM AITKEN, M. D., Edinburgh, Professor of Pathology in the Army Medical School; Corresponding Member of the Royal Imperial Society of Physicians of Vienna; of the Society of Medicine and Natural History of Dresden, and of the Imperial Society of Medicine of Constantinople; late Pathologist attached to the Military Hospitals of the British troops at Scutari, and formerly Demonstrator of Anatomy in the University of Glasgow.

This is the first volume of an apparently able work on Practical Medicine. It is from the fourth London edition, and has additions by Meredith Clymer M. D., late Professor of the Institutes and Practice of Medicine in the University of New York, formerly Consulting Physician to the Philadelphia Hospital, etc. Published by Lindsay & Blakiston, Philadelphia, 1866.

For sale by James M. Crawford, No. 54 Fourth street, St. Louis, Missouri.

PEREIRA'S MATERIA MEDICA AND THERAPEUTICS—WOOD; BEING AN ABRIDGEMENT OF THE LATE DR. PEREIRA'S ELEMENTS OF MATERIA MEDICA. By FREDERIC JOHN FARRE, M. D., Cantab., F. L. S., Fellow of the Royal College of Physicians, London; Honorary Member of the Pharmaceutical Society of Great Britain; Examiner in Materia Medica and Pharmacy to the University of London; Lecturer in Materia Medica in St. Bartholomew's College, etc., etc. Assisted by ROBERT BENTLEY, M. R. C. S., F. L. S., Honorary Fellow of King's College, London, Professor of Botany in King's College, and Professor of Materia Medica and Botany to the Pharmaceutical Society of Great Britain; and by ROBERT WARRINGTON, F. R. S., F. C. S., Chemical Operator to the Society of Apothecaries, and Vice-President of the Chemical Society. Edited with

numerous references to the U. S. Pharmacopœia, and many other additions. By HORATIO C. WOOD, Jr., M. D., Professor of Botany in the University of Pennsylvania, Auxiliary Faculty of Medicine, etc., etc. Two hundred and thirty-six wood engravings. Philadelphia: Henry C. Lea, 1866.

For sale by E. P. Gray, Bookseller, northwest corner of Fourth and Locust streets, St. Louis, Mo.

PRACTICAL THERAPEUTICS. CONSIDERED CHIEFLY WITH REFERENCE TO ARTICLES OF THE MATERIA MEDICA. By EDWARD JOHN WARING. F. R. C. S., F. L. S., Surgeon in Her Majesty's Indian Army. From the second London edition. Philadelphia: Lindsay & Blakiston, 1866.

This is a volume of 815 pages, in beautiful type, and binding in cloth. It contains 672 pages on the Materia Medica, and 102 pages of Practical Therapeutics, with a full index of diseases and of medicines. It contains all the preparations of the new British Pharmacopœia, with notices of the principal new remedies which have been introduced into practice since the edition of 1854. For sale by James M. Crawford, Bookseller, No. 54 Fourth street, St. Louis.

REPORT ON THE SANITARY RELATIONS OF THE STATE OF KANSAS. By C. A. LOGAN, M. D. Lawrence, 1866.

This is a well written report, and full of useful information on hygiene. It contains fifty pages of close matter, that will repay the reader his care in the perusal.

TRANSACTIONS OF THE TWENTY-FIRST ANNUAL MEETING OF THE OHIO STATE MEDICAL SOCIETY, held July 19th, 20th and 21st, 1866.

It contains a very excellent valedictory, by B. S. Brown, M. D., retiring President.

A REPORT ON MILITARY SURGERY. By NORMAN GAY, M.D., of Columbus. On Amputations and Resections.

A REPORT ON OBSTETRICS. By THADDEUS A. REAMY, M. D., of Zanesville, Ohio.

THERAPEUTICS OF ZYMOTIC DISEASES. By EDWARD B. STEVENS, of Cincinnati. A very able report.

REPORT OF A CASE OF FOREIGN BODY IN THE FEMALE BLADDER. By W. C. HALL, Fayetteville, Ohio.

REPORT OF THE INFLUENCE OF PUBLIC SCHOOLS ON HEALTH. By ROBERT M. DORING, M. D., Columbus, Ohio.

REPORT OF COMMITTEE AS TO THE NUMBER AND CONDITION OF INCURABLE INSANE IN THE DIFFERENT COUNTIES OF OHIO. By E. PEARCE, M.D., Steubenville, Ohio. B. S. BROWN, M. D., Bellefontaine, Ohio.

There is very much in these transactions worthy of notice, and they should be in the hand of every physician.

ORTHOPEDICS: A SYSTEMATIC TREATISE UPON THE PREVENTION AND CORRECTION OF DEFORMITIES. By DAVID PRINCE, M. D., Philadelphia: Lindsay & Blackiston, 1866.

This is a very neatly gotten up work of 240 pages, and will do credit to the already extended reputation of its able author. We will have occasion to notice it more fully in the next issue of the Journal.

For sale by Keith & Woods, Fifth street, St. Louis, Mo.

ASIATIC CHOLERA; A TREATISE ON ITS ORIGIN, PATHOLOGY, TREATMENT, AND CURE. By E. WHITNEY, M. D., and A. B. WHITNEY, A. M., M. D. New York: M. W. Dodd, Publisher, No. 506 Broadway, 1866.

CEREBRO-SPINAL MENINGITIS: A REPORT MADE TO THE ILLINOIS STATE MEDICAL SOCIETY, JUNE, 1866. By J. S. JEWELL, M. D., Professor of Anatomy in the Chicago Medical College, etc. George H. Fergus, Clark street, Chicago.

A PRACTICAL TREATISE ON FRACTURES AND DISLOCATIONS. By FRANK HASTINGS HAMILTON, A. B., A. M., M. D., Professor of the Principles of Surgery, Military Surgery, and Hygiene, and of Fractures and Dislocations in Bellevue Hospital and Medical College; Professor of Military Surgery, etc., in the Long Island College Hospital; author of Treatise on Military Surgery, etc., etc., etc. Third edition. Revised and Improved. Illustrated with 294 wood cuts. Philadelphia: Henry C. Lea, Publisher, 1866.

For sale by Keith & Woods, Fifth street, St. Louis, Mo.

A HAND BOOK OF OPHTHALMIC SURGERY, FOR THE USE OF PRACTITIONERS. By JOHN Z. LAWRENCE, F. R. C. S., M. B., Surgeon to the Ophthalmic Hospital, Southwark; Editor of the Ophthalmic Review; Member of the Heidelberg Ophthalmological Society; of the University Ophthalmological Society of London; of the Harveian Society; of the Society of Practical Medicine, Paris; of the Society of German Naturalists and Physicians; and by ROBERT C. MOORE, House Surgeon of the Ophthalmic Hospital, Southwark. With numerous illustrations. Philadelphia: Henry C. Lea, Publisher, 1866.

For sale by Keith & Woods, corner of Fifth and Olive streets.

The following exchanges have been received:

American Journal of Medical Sciences.
The Richmond Medical Journal.
The New York Medical Journal.
The Medical and Surgical Monthly, Memphis, Tennessee.
The Galveston Medical Journal.
The Medical Record, New York.
The Savannah Journal of Medicine.
The Pacific Medical and Surgical Journal.
Southern Journal of the Medical Sciences.
The Cincinnati Lancet and Observer.
The Journal of Materia Medica.
The Medical and Surgical Reporter, Philadelphia.
The New Orleans Medical Record.
The Boston Medical and Surgical Journal.
The Medical News and Library, Philadelphia.
American Journal of Pharmacy.
American Journal of Insanity.
The Druggists' Circular.
The Chicago Medical Examiner.
The New York Lancet.
The Chicago Medical Journal.
Atlanta Medical and Surgical Journal.
The Dental Cosmos.
The Cincinnati Journal of Medicine.
The Dental Register.
The Detroit Review of Medicine and Pharmacy.
The Canada Medical Journal.
The Buffalo Medical and Surgical Journal.
The Dissector, Jerseyville, Illinois.
Journal of Applied Chemistry, New York city.
Northwestern Christian Advocate, Chicago.
Braithwaite's Retrospect of Medicine, New York.
Journal of Ophthalmology, New York city.
The American Eclectic Medical Review.
The Philadelphia University Medical Journal.
Homeopathic Expositor, Milwaukee, Wisconsin.

MISCELLANY.

CRIMINAL ABORTION.

We abstract from the able report by Dr. Thaddeus A. Reamy, M. D., of Zanesville, made to the Ohio State Medical Society, the appropriate words on this interesting subject, that has of late attained such criminal proportions. The following is only a small part of his worthy report:

Criminal Abortion. This subject is not introduced from choice. It is a painful duty which I can not refrain from discharging. Six years ago one of the most acute observers and one of the ablest members of this Society, then Chairman of Committee on Obstetrics, when speaking in his report of this evil, made the following prediction: "The time is not far distant when children will be sacrificed among us with as little hesitation as among the Hindoos." That prediction seemed to me at the time it was uttered, as it doubtless did to many of you, an extravagant one. But six short years have almost brought its literal fulfillment. Great God! What a state of morals for a Christian people, boasting a high degree of civilization. In my correspondence upon this subject I have found considerable reticence among medical men; many are inclined to evade a direct answer. This grows out of the delicacy of the subject, doubtless. But from a very large verbal and written correspondence in this and other States, together with personal investigation and facts accumulated, I am convinced that we have become a nation of murderers. And the victims are innocent, helpless children! Things may as well be called by their proper names. This practice is much more prevalent in the cities than in the rural districts; nevertheless, even in the country to a frightful degree. The physician is consulted as to how pregnancy can be avoided; or if it has already occurred, how it can be got rid of. With as much coolness and business-like composure, and that by the fathers or the mothers, as would characterize a consultation

with a gardener as to plucking weeds from among flowers. I doubt whether there are a half dozen practitioners present to-day who have not been consulted within the past year repeatedly in such cases. "We have as many children as we want," "my health is so poor I can not bear children," etc., are phrases familiar to every physician. And be it remembered, these applicants are not confined tc the poor, the indigent, or the ignorant; they are from all classes. This is one of the most alarming features of the case. The mania to destroy has gone into every avenue of society. The lettered and unlettered, rich and poor, professed christian and sinner, throng in one great mass the bloody altars, and sacrifice their offspring.

The deity who presides at such an altar ought to be carved in brass, and a copy furnished to every household which has been, or is likely to be cursed with such worship. It ought to represent a woman in the act of strangling an innocent, sweet-countenanced child; her features contorted with the heartless visage of her cruel purpose; and the second figure should represent her husband, or some lady friend, plugging her ears lest the cries of the child might move her to sympathy. Her eyes should be intently fixed upon an object in the distance, that she may not catch the countenance of the infant and behold in it the image of God, and thus be deterred from her bloody purpose. If a physician informs them when applied to that he can not consent to such a thing, that it is murder, he will quite likely be informed that if he will not some others will; and it is to be greatly feared others do. Women learn these things from each other; many of them follow it for themselves and friends. They will use knitting needles, whalebones, a catheter, or some other instrument—rupturing the membrane. And the practice is not confined to the first three or four months of pregnancy. So intent are they often that if success has not attended their efforts sooner, when quickening occurs, instead of having a mother's sympathy aroused, and solicitude for the welfare of her offspring, she is only reminded that she must be prompt in dispatching it before it is larger to give her trouble.

The bloody command of Pharaoh to the Hebrew midwives, that when they did the "office of midwife to the Hebrew women and saw them upon the stool, if it were a son they should kill him," has always called upon the head of that king our execrations; he has been held as a heartless monster. We have looked with horror upon the practice of the Romans in the dark ages destroying their offspring, both before and after birth, as portrayed by Juvenal in his Sixth Satire; and we have rejoiced as the mild influence of Christian civilization has controlled and enlightened the minds of the people and abolished this custom. We have wept over the heathen mother who throws her child into the Ganges as a sacrifice to her imaginary God; and we resolved to give liberally to the missionary fund that she might have the light of a better gospel. And we have made similar resolves as we read the accounts of the baby tamers in China and India. Pharaoh was a wicked and ambitious king, who had natural enmity against the Hebrews, and was jealous lest they who had been his slaves might become his conqueror. This bloody edict was therefore one of national policy. The Romans and Chinese were ignorant heathens, who knew but little of the claims of humanity, and nothing of the obligations of the Christian religion.

But what shall be said of Christian mothers murdering their offspring without cause and without conscience? And what of physicians who lend their influence and counsel in behalf of such deeds? This earth is far from being regenerated. The dark waters of the Ganges foam with the struggling of heathen infants offered in sacrifice; the bones of thousands of murdered children are bleaching at the base of the baby tamers in China. But in Christendom, yea, in our own beautiful Ohio, there prattle around the hearthstones of cottages and palaces, children who have dead infant brothers and sisters, hidden and unnamed, who were murdered by their parents' hand. What mockery for the voice of evening and morning prayer to be heard, asking God to bless and protect "Willie" and "Ellie," while from the same heart from which goes out the prayer went out the determination to destroy an unborn child! In many instances the pages of the sacred

volume are turned for the reading of the morning lesson by the very hand that so recently directed the weapon for destruction. Can God hear with favor the prayer that goes up incensed with the blood of murdered innocence? There are but few communion boards in this or perhaps any other country which have not among those who surround them at every season persons guilty of this crime.

But this subject is too revolting to contemplate. I must leave it. No doubt, some will think I have overdrawn it, or given too much time to it.

I know it is not overdrawn; and if not, too much importance can not be attached to it. It is blighting the health of thousands of women; causing ulcerations, displacements, and inflammation of the uterus. In its secondary results, it is one of the most prolific sources of moral prostitution. It is with fearful rapidity prostituting woman from the noble and God-given embodiment of virtue, innocence, and affection, which has and always should make her an object of adoration and tender care, to a cruel monster whose level is far below the brute. It is filling our countries with mothers who have either committed suicide and infanticide or secured the services of a friend more skillful than themselves. I once attended a woman, wife of an intelligent, kind-hearted husband in good circumstances, mother of three lovely children, a devoted and prominent member of a Christian Church. She aborted, despite all my efforts to prevent it. She was over four months advanced. Grave symptoms supervened; purulent absorption was manifest. She died on the sixteenth day. An autopsy was had by myself and a medical friend. She had perforated the uterus very near the os with a wire which was in a male gum catheter, the wire having been thrust through the catheter in her violent efforts to destroy the fœtus. The veins throughout the whole system were filled with pus. She confessed to me, on the third day of her illness, that she had brought on the abortion in the way named. I attended her funeral; her minister preached her to the pure Paradise of God. She had committed murder and suicide. The custom is making homes that

ought to echo with the matchless music of prattling children silent as the murderer's cave; the air that should be fragrant with the flowers of their planting and care, is loaded with the stench of detached and putrid ova. In short, the practice is eating as a cankering mildew at the very vitals of our holy Christianity, and unless speedily checked will damn us as individual communities and as a nation.

What can be done? I answer, much. Medical men, from their intimate relations to the families which they attend, and from their acknowledged acquaintance with the whole subject, can command respect for their counsels in these matters above all others. Let every honorable member of this Society, and of the entire profession, whenever consulted by a woman on this subject, candidly and patiently explain to her the nature of the crime she contemplates—set it before her in its true character.

The opinion has become prevalent among women, that if the child be destroyed before quickening, no sin attaches to the act; and that if the separation of the fœtus takes place after quickening, but before viability, though it may be somewhat sinful, yet it does not amount to much. If they are counseled properly against such fallacies, they will generally desist at once. Medical men should inform them candidly and forcibly that such an act is murder at any stage of pregnancy. I am thoroughly convinced with Dr. Storer, of Boston (Prize Essay), "that the employment, in the married relation, of any means whatever, for preventing conception, is but little else than legalized prostitution. Let no physician who claims or expects to claim, respect from God or his fellow men, be either directly or by advice, party to such an atrocity. And let every member of the profession who has already acted, or will in future act, upon these suggestions, constitute himself a vigilance committee, to bring to justice and condign punishment any and every medical man in his community who degrades himself to such an office. Let us all refuse to subscribe for and receive at our firesides, newspapers, secular or religious, in whose columns appear advertisements of patent nostrums for "removing all irregularities, from whatever cause

produced." "The proprietors would advise against women, who are in a certain condition, taking the medicine, etc., as miscarriage would occur, although even then there would be no danger." Much has been said, and well said, by the editor of the Medical and Surgical Reporter, Philadelphia, against the evils I have been trying to portray. This journal, an excellent one, should have our warmest commendation for its outspoken fearlessness in condemning this business. Our Western journals should also sound the alarm. Dr. Houghton, in an Obstetric report made before the Wayne County Medical Society, during the year, speaks out also in words of bold rebuke. Let every conscientious physician speak out, and every medical journal in the land chronicle the evil. Thus can our profession come to the rescue, and signally co-operate with all other agencies of our holy Christianity in saving society from imminent impending physical, moral, and spiritual ruin. Note. Criminal prosecutions for abortions procured upon unmarried females for concealing shame, murder of the new born infant, by neglect or otherwise, for the same purposes, etc., must all cease, unless abortions among married women for convenience can be brought to an end. I can see no propriety in continuing any jurisprudence over the subject at all, unless the community is relieved of the demoralizing effects of this crime; it would soon be difficult to find a jury that would convict even in the clearest case.

"THE PRESENT STATE OF THE DOCTRINE OE FEVER."

The following communication from Prof. J. H. Watters, M. D., was received too late for insertion among the Original Communications. His references to certain points in the article of Dr. Baumgarten on "The Present State of the Doctrine of Fever," in the last number, indicate the importance of that article and the subject. We give it a place among our editorial items.

In reference to "The Present State of the Doctrine of Fever," as presented by my friend, Dr. Baumgarten, in the last number of the *Journal*, representing the views of Liebermeister, Weber,

and others, I do not deem it out of the way to make one suggestion: They hold that "fever consists in a general pathological increase of organic change," and that the increased heat of fever is due to this increased change, and that the dangers to be apprehended are attributable directly to this elevation of temperature which is the common element. With all due respect I would suggest this inquiry: May it not be that *all* the various organic processes and vital changes, together with the heat, represent and express as a totality this "organic change," or destructive metamorphosis, referred to? If this is *possible*, then the conclusions of Liebermeister, Weber, and others, do not follow. For then, if the *vital processes* be interfered with by the altered conditions of disease, we might have an elevation of temperature with even less destructive metamorphosis. To illustrate: Every man of science thinks he understands why water in an open vessel can not be heated above 212° F., however rapid the destructive metamorphosis of wood or coal. Now if a film of oil be upon the surface of the water, or if any cause interferes with the formation of steam, the water will rise in temperature above 212° F. *This* increased heat of the water does not represent an increased heat of the fire, but rather an interference with the free formation of steam. So if the various vital processes, no less than heat, and "radiation, and evaporation," represent the "oxydation," then the increased heat of fever *might* rather represent an interference with the vital processes by the abnormal conditions; and the great danger to be apprehended when the fever-heat is great, might be due to these altered vital conditions which the increased heat merely *indicates*, rather than to the heat itself directly.

Astronomy took its great start from more precise conceptions of the laws of motion, and physicists are now advancing rapidly to more exact conceptions of "force," which it will not well do for physiologists and pathologists much longer to ignore. This, too, is a *prelude*. Now, let that class which forced Galileo to recant, and which caused witches to be burned (for superstition is the same) look to their "occupation." The handwriting is already on the wall; let those who have eyes to see, read. Selah!

We clip the following appropriate letter, by R. H. Paddock, M. D. (regarding City Hospitals), from a city paper. It is worthy of a careful perusal:

A RESIDENT PHYSICIAN'S EXPERIENCE IN THE CITY HOSPITAL—A CURIOUS REVELATION.—The following communication was taken up by the Board of Health at its last meeting, and referred to a special committee, consisting of Messrs. Krieger and Belt. The communication endeavors to give an inside view of the City Hospital, and will be best understood, as Hans Patrick Le Connor would say, by reading it:

Having had several years experience in the management of both civil and military hospitals, I avail myself of this opportunity to make some remarks upon the mode of conducting hospitals in general, this one in particular. I should have done this before this time, only that my motives for so doing might have been misconstrued. I have now resigned my place, and henceforth neither expect nor desire ever again to share either the emoluments or honors that attach themselves to any city office.

First, then, allow me to say that the plan upon which the hospital is conducted is inherently mischievous and bad, while the ends sought and intended to be accomplished by the institution—the care and cure of the sick and wounded poor—are eminently creditable and praiseworthy. The city authorities appoint a man, styled the Resident Physician, to come here and live, and require him to give his whole time and talents to the business of the house. They also allow him to have as many persons to aid, assist, and labor in the house as are needed. This is all well, and as it should be; and had the city fathers gone on and perfected their work, the hospital, as it seems to me, would have been organized as it should be. What was needed in addition to their work was an ordinance giving the Resident Physician the authority to select, employ, and discharge at his discretion, the various employes of the house, to designate the duties and spheres of each, and to hold every one accountable directly to himself for the faithful performance of the part assigned him; to make him in fact as well as in form the Superintendent of the house. Then he could afford to be held responsible for the good condition and conduct of the house, to any suitable Board of Visitors and Inspectors which the city might appoint, and then would he receive suitable commendation when he did well, and merited reproof or removal when he failed to do well. He might, also, through his steward (who should be a man of business capacity, and good education and character), take charge of the buildings, grounds, supplies, and financial affairs of the institution; and the auditors of his accounts, appointed by the city, would see that all was done prudently and economically. Such is the plan upon which all military hospitals are operated, and it will be found difficult to devise a better one. The Government of the United States gives to the surgeon in charge of a hospital the entire control of all its affairs, whether the hospital contains hundreds or thousands of patients, and with the happiest results. The ordinances of the city, as they now stand, limit

the power of the Resident Physician to a proper control of the medical department of the hospital, and to a joint action of the Board of Health in making the "necessary rules and regulations for its government and management." The general supervision and management is given exclusively to the Board of Health. Now, in most cities, the Board of Health is charged only with the duty of attending to cleanliness and general sanitary matters and measures, and not with conducting hospitals, which is obviously a business suitable for physicians only. Besides, this Board consists of five or six members, and no institution can be actually conducted by several persons, neither of whom resides in it, and all of whom are engaged in other occupations. Hence this Board *must* delegate its authority to some *one* or *more* persons, to act for it. In point of fact this authority is partially delegated to several different persons, but to no one in full, the Board still acting directly itself, from time to time, as it sees fit.

Now, such a mixed administration of one house—partly by the Resident Physician, partly by the Board of Health, and partly by one-half dozen appointees of the Board—can not be harmonious and salutary. Nature never gives but one head to one animal. Wise men place one man at the head of each of their various business establishments. Ships and steamboats have each but one captain, and any one house, public or private, which has more heads than one, will not be well conducted. The past history of this house is just what might be expected from its mixed administration, and it is just what its future history will be, until *some one person* shall preside over it with plenary powers.

When I first came to this house I found persons employed in it who ought not to be here; persons incompetent, unreliable, mischievous and dishonest; but all contrive to cover their faults from the Board of Health, which could not be here to see and know for themselves. But woe to the man who should impose upon them. They all had their friends and allies, both in the house and out of it, and even in the Board, and a controversy with those who had neither integrity nor character to lose, could not fail to result disastrously to any person who had either to maintain. A little experience in the house taught me that certainly "prudence was the better part of valor." Until some person shall be placed here with plenary powers over all employed, such evil persons will find their way into the house, and will contrive to keep their places quite too long through such aids, influences and plottings as they can devise.

There is one more topic to which I wish to call the attention of the Board, Heretofore the only business of the house in which the Resident Physician has exercised supreme and undisputed authority, has been the strictly medical and surgical business. Both the city ordinances and the standing rules of the Board of Health give him entire control here; and, until quite recently, no attempt has ever been made to interfere with his medical practice or surgical operations, or to instruct him what physicians he should allow to see his patients, or to assist him in making his surgical operations, or to give him their opinions and advice. The only condition ever imposed

upon him, either by ordinance or rule, was, that before making any capital sugical operation, he should hold a consultation upon the case, in order to decide upon the necessity and propriety of the operation, with one or more of the consulting physicians of the house. These consulting physicians consist of six medical gentlemen of the city, named by the Board annually *as a compliment*, who are expected to advise and assist the Resident Physician when he shall request their advice or assistance; and at no other time, and never without invitation. They receive no compensation for their services, and assume no legal responsibility.

Recently an effort has been made through the agency of this Board to take away from the Resident Physician all the more important surgical operations, for the skillful performance of which he alone is legally responsible, and to place them in the hands of the professors of two medical colleges. It is not presumed that this effort originated with any member of this Honorable Board. The nature of the effort clearly betrays its origin, and it will surprise no one who knows its origin, that such an effort should emenate from such a source. Least of all does it surprise the Resident Physician, who has for years been annoyed by the persistent efforts of a certain physician to force his services upon this house when not desired, in order to display the wonderful surgical ability of which he imagines himself to be possessed. A member of this Board was induced, doubtless by outside representations and misrepresentations, to lend his aid to this effort, without taking the precaution to make the inquiry requisite to ascertain *all* the facts in the case. This effort assumed the form of four resolutions, all of which, to say the least of them, were uncalled for, unkind, and discourteous, both to the Resident Physician, to one principal medical college, and to one-half of the consulting physicians of the hospital.

Will the Board please direct their clerk to read the four resolutions introduced by Mr. King, and adopted April 30, 1866?

The first resolution directs the Resident Physician to observe the ordinance forbidding him to call in consultation any other than some of the consulting physicians appointed by the Board of Health.

Answer—There is no such city ordinance; there is no such rule in the published standing rules of the Board, nor in any other place, as far as I know. There never should be a rule forbidding any physician, either in public or private, from giving his patients the benefit of the best advice he can obtain. Under the operation of such a rule as this the three members of the Humboldt Faculty, who now compose one-half of the corps of consulting physicians, would have been excluded the last year, and may be the very next.

The second resolution directs the Resident Physician to show no partiality or preference for one consulting physician rather than another, but to call on all of them alike for assistance.

Answer—He should ask advice of such physicians as are most competent to give it, and this he always has done. Besides, the very next resolution of the series commands them to violate *this* resolution, for it directs him to

give *all* his important surgical cases to three of the consulting physicians, and consequently leaves *none* for the other three.

The third resolution asserts that all capital surgical operations shall be performed during eight months of the year by two of the professors in the Humboldt School, and during the remaining four months, by one of the professors in the St. Louis School.

Answer—Supposing the Board had the authority to make such an arrangement, we can not but wonder at its unfairness! During the period when the old St. Louis School, with its *two hundred students*, is in session, the hospital is given to the new Humboldt School, with its eight or ten students, and during the remainder of the year, when the St. Louis School is not in session, and has no students, it has the privilege of the hospital. Comment is unnecessary here.

The fourth and last of these remarkable resolutions provides that when Humboldt Professor No. 2 is engaged, he may if he please employ Humboldt No. 1 to take his place for four months, or any shorter period, so as to secure all the surgery for Humboldt for the whole eight months; also, that should the Resident Physician ever presume to make an important surgical operation, he is to notify the three Humboldt Professors of the time, that they may be present to oversee him.

Gentlemen, enough has been said of this effort to interfere with the legitimate business of the Resident Physician of the hospital. It is well understood that the Board passed these resolutions hastily and without understanding their full import. There is and has been no hostility on the part of the Resident Physician to any legitimate and honorable medical school. He has himself been a professor in medical colleges for about sixteen years, and knows the value of hospital facilities for medical students.

When the Humboldt School was started last March its professors were kindly received at the Hospital, and every facility granted them which had ever been granted to the old college, and even more; only its surgeons were not invited to make operations for the Resident Physician. No professor ever before requested, even much less *demanded*, this privilege. It has been *granted*, it is true, *as a courtesy*, or a *compliment unasked*, but it *should not and can not be extorted*. That an effort has been made to do so, only shows the truth of what was stated at the outset; viz., that the *plan* upon which the hospital is conducted is vicious and faulty, and that it needs one acknowledged head to conduct all its affairs. Such a head, it is hoped, may hereafter be found. In fine, gentlemen of the Board, if my advice in reference to the management of the hospital be considered worth anything, it is briefly this: Instead of substracting anything from the honors, emoluments, powers, duties, and responsibilities of the Resident Physician, much should be added to each of them; he should have entire control of everything about the house, and should be then held to a strict account for the good conduct and economical administration of the same. Then it will be possible to command the service of a man who is competent and worthy to be trusted, and not till then will any such man be willing, for a long time, to fill a place so circumscribed and subject to annoyance as this is at present.

In conclusion, I wish to return my thanks to all those distinguished and able medical gentlemen who have kindly and gratuitously aided and advised me during my term of service here, and especially to Drs. C. A. Pope and J. T. Hodgen, one of whom has visited the house nearly every week at my request. To my assistants also, Drs. H. H. Mudd and A. F. Einbeck, I am largely indebted for their faithfulness, assiduity, and unpaid toil, and to the Manager and Matron of the Hospital, as well as to all subordinate officers.

I wish to express my kind regards and best wishes, finally, to the members of the Honorable Board, who have extended to me kindness, and courtesy, and hospitality. I wish to make all suitable acknowledgements.

Very respectfully,

H. R. PADDOCK, *Resident Physician.*

We abstract the following editorial from an Omaha paper, giving an interesting case of wound from arrows, treated and reported by our old friend, whom many of our readers well know:

At our request, Dr. J. H. Peabody, of this city, has furnished us with the following statement of an interesting and delicate surgical operation performed by him last Thursday,—one which reflects great credit upon his professional skill. We wish to state distinctly that Dr. P. has furnished the statement at our earnest request, we making the request in the belief that it was a case the particulars of which would be worthy of publication and of interest to our readers.

George Osborn, company D, Second Nebraska Volunteer Cavalry, was wounded severely, near Pawnee Reservation, on the 22d of June, 1868 (more than three years since). He received eight arrows in various parts of his body, which were all extracted except the one mentioned below.

Mr. Osborn called at my office Thursday evening last for examination, with a view to applying for a pension, stating that his health was much affected from an arrow head, which he believed was in his right lung, from the fact that when he coughed it caused great pain, and that he swallowed with difficulty. Upon examination, I found the arrow had entered at the outer side and lower margin of the right scapula (shoulder blade), passing upwards and inwards through the upper lobe of the lung or trachea (judging from the amount of hemorrhage reported as occuring from the mouth at the time of injury). Upon probing through an opening made in the neck, just above the superior end of the sternum, I found the point of the arrow, apparently firmly fixed, about an inch and a half below the superior end, the head lying flat against the trachea and œsophagus, with the carotid artery, jugular vein, and nerves, overlaying it. With some little fear of cutting the carotid artery, having to tie the innominate, or perhaps worse, of having the patient bleed to death in my office, I proceeded to draw the arrow from its lodging place, and was as much

pleased as my patient to have it safely in my hand without the loss of an ounce of blood.

Upon measurement it was found to be three-quarters of an inch at the base, by four and a quarter inches long; rather an unpleasant missile to be so long lying among the most delicate arteries, nerves and organs of a man's body.

Mr. Osborn rode to his home in Sarpy county after the operation was performed. He expressed himself under many obligations to me, appearing highly gratified to get rid of his Indian keep-sake.

Since the article of Dr. Montgomery was in type, we have received the following postscript, which we now publish in the editorial columns:

P. S.—Since the above was written, the epidemic has passed through our city, and the further experience and observation of the disease more strongly confirms my faith in the views above enunciated.

The prompt and efficient administration of the emetic in the commencement, followed by small doses of some of the mercurial preparations every half hour, until bile appeared in the stools; an occasional dose of bismuth and creosote, or the permanganate of potassa and glycerine; a few drops of phosphoric, nitric, or sulphuric acid in a wine-glass of ice-water, as a drink; injections of quinine and iron, or permanganate of potassa in large quantities of tepid water, with external frictions with hartshorn and turpentine, were the principal remedies I employed; and on taking a retrospective view of the means adopted by others, I can see no reason to change my therapeutics. I only regret that I did not begin the use of the permanganate of potassa and glycerine until the decline of the epidemic, as the cases in which these remedies were employed seemed to be much benefited by them.

I see from the *Medical Times and Gazette*, of August last, that Dr. Lionel Beale, the great physiologist and microscopist, has been instituting experiments and researches on cholera, and he clearly demonstrates, to my mind, that the chief cause of collapse

and death is the detachment of the epithelial coating of the intestinal villi. Now, this tender, raw, and denuded surface of the intestinal membrane, with the consequent partial destruction of the powers of absorption should certainly admonish us to refrain from the use of strong astringents and stimulants in this sthenic disease, whilst the well known soothing and healing qualities of bismuth, permanganate of potassa, glycerine, etc., strongly commend these remedies to our most sedulous consideration.

E. MONTGOMERY.

St. Louis, October 17, 1866.

We notice in an announcement, that the *Weekly Hesperian*, "a journal devoted to the literary interests of the Mississippi Valley," will be issued on Saturday, January 5th, 1867. It will be á weekly, containing essays, romances, sketches, and poems, original and selected, and will be "in eight page form, and size of New York *Ledger;* will contain forty columns of reading matter, at three dollars a year in advance." Published by Jas. W. Allen, St. Louis, Mo.

St. Louis, Mo., Oct. 11, 1866.

F. W. White, M. D.

Dear Sir:—At the Annual Meeting of the St. Louis College of Pharmacy, held at their hall, Monday, Sept. 24th, 1866, the following officers were elected:

Enno Sander, President.
W. H. Crawford, First Vice-President.
E. L. Massot, Second Vice-President.
H. Prim, Recording Secretary.
E. J. Williamson, Corresponding Secretary.
Dr. C. L. Lipps, Treasurer.

The College opened its session with about the same class as last year, on the 1st inst.

Very respectfully yours,

E. J. WILLIAMSON, *Cor. Sec.*

TO OUR PATRONS.

It is gratifying to acknowledge your cordial co-operation during the past year. *The circulation of the Journal has been almost doubled.* For this success we have none but our patrons to thank. Promising that the excellence of the *Journal* shall be commensurate with the support it receives, and on which it must depend, we invite all our friends to send forward their CASH subscriptions.

THE COMING VOLUME—1867.

The volume for 1867 will be in size as former volumes—*at the same low price—with the addition of sixteen pages* each alternate month, as a supplement, containing News Items, Transactions of the St. Louis Medical Society, St. Louis Academy of Sciences, and County Medical Societies that may favor us with reports. Subscribers who have sent us five dollars for 1867, will be returned the balance, or credited for next year, at their option.

Our readers must bear with us if the *Journal* does not always arrive when it should, since any failure to do so results from causes wholly unavoidable on our part. We *know* that every number has been mailed regularly to all subscribers not in arrears, and several numbers to those who were. As far as possible, missing numbers will be supplied to such of our patrons as make seasonable payment of their subscriptions.

ERRATUM.

In table No. 2 there occurred a mistake under the head of "Absolute Myopia," which should have read thus: 3. Extreme Myopia, or Hypermyopia. This has induced us to reprint the whole table, a copy of which we send to our subscribers.

OBITUARY.

MILTON KYLE, M. D.

At a meeting of the St. Louis Medical Society, held September 29th, 1866, the following preamble and resolutions were unanimously adopted:

WHEREAS, Our late associate, Milton Kyle, M. D., has recently died of consumption, after a brief practice of his profession; therefore,

Resolved, That the manliness of his character, which endeared him to a large circle of friends, his faithful work already done, and the promise that he gave of a future successful professional career, alike impel us to make this public expression of our regard for his loss.

Resolved, That we dwell with satisfaction upon the thought, that although his life was short, yet he rendered much arduous and valuable service to the sick and wounded.

Resolved, That we respectfully tender to the family of our deceased friend, our heartfelt sympathy in their irreparable bereavement.

WM. JOHNSTON, M. D., *President.*

R. S. ANDERSON, M. D., *Recording Secretary.*

DANIEL BRAINARD, M. D.

At a meeting of the St. Louis Medical Society, held Oct. 11th, 1866, Dr. M. L. Linton announced to the Society the death of Dr. Brainard, of Chicago. He alluded to the early connection of the deceased with the St. Louis Medical College, as Professor of Anatomy—to the high-toned character and great professional ability displayed by him in that position. He related several incidents occurring in his early life in this city, and regretted sincerely the great loss sustained by the medical profession of the United States in the death of so eminent and excellent a man. After which he offered the following resolutions, which were unanimously adopted:

1*st.* *Resolved*, That the members of this Society deplore the death of Dr. Brainard, of Chicago, as a national professional loss.

2*d.* *Resolved*, That as a surgical teacher and operator, Dr. Brainard stood in the front rank of his profession.

3*d.* *Resolved*, That the bereaved family of the deceased has the sympathy of this Society.

4*th.* *Resolved*, That these resolutions be published in the daily papers of this city.

Dr. Hammer seconded the resolutions, and said that he regretted deeply that death had deprived the medical profession of one who was at once so much the gentlemen and the surgeon. He related several anecdotes of Dr. Brainard, which occurred during his connection with him as a student in Paris, and which were indicative both of his kindness of heart and his professional attainments.

WM. JOHNSTON, M. D., *President.*

R. S. ANDERSON, M. D., *Recording Secretary.*

MORTUARY STATISTICS

For the city of St. Louis, Mo., for the month of September and October, 1866.

SEPTEMBER.—Apoplexy, 1; Atrophia, 9; Abscess, 3; Accident, 8; Burn, 1; Bronchitis, 4; Congestive Chills, 6; Cholera Epidemic, 900; Cholera Morbus, 178; Cholera Infantum, 111; Cerebritis, 1; Convulsions, 42; Congestion of Brain, 24; Congestion of Lungs, 9; Cramp, 15; Congestive Fever, 8; Croup, 1; Cancer, 2; Catarrh, 1; Concussion of Brain, 1; Congestion of Stomach, 2; Diarrhœa Acute, 8; Diarrhœa Chronic, 50; Dentition, 10; Debility, 42; Diphtheria, 7; Drowned, 1; Dysentery Acute, 14; Dysentery Chonic, 28; Dropsy, 5; Delirium Tremens, 4; Dyspepsia, 2; Epilepsy, 1; Eclampsia, 5; Enteritis, 8; Febris, 9; Gastritis, 13; Heart Disease, 6; Hemorrhage of Lungs, 3; Hydrocephalus, 14; Hepatitis, 5; Intermittent Fever, 5; Inflammation of Bowels, 12; Inflammation of Brain, 9; Jaundice, 1; Marasmus, 86; Meningitis, 16; Measles, 2; Pneumonia, 38; Peritonitis, 2; Phthisis, 20; Paralysis, 2; Pertussis, 3; Poison, 3; Premature Birth, 7; Remittent Fever, 1; Rheumatism, 1; Still Born, 45; Syphilis, 5; Suicide, 2; Scorbute, 1; Spasms, 14; Summer Complaint, 15; Scrofula, 1; Tuberculosis, 2; Tetanus, 2; Typhoid Fever, 78; Trismus, 12; Tabes, 3; Ulceration of Bowels, 1; Unknown, 29; Whooping Cough, 7; Wound, 5. Total, 1943.

United States, 1282; Germany, 375; Ireland, 218; Italy, 5; Bohemia, 16; England, 18; Scotland, 9; Prussia, 7; Canada, 8; France, 7. Total, 1943.

OCTOBER.—Apoplexy, 2; Asthma, 2; Abscess, 3; Accident, 15; Burn, 1; Bronchitis, 4; Congestive Chills, 4; Cholera Epidemic, 64; Cholera Morbus, 10; Cholera Infantum, 8; Cerebritis, 3; Convulsions, 37; Congestion of Brain, 11; Congestion of Lungs, 3; Cramp, 2; Congestive Fever, 3; Croup, 2; Cancer, 1; Catarrh, 1; Concussion of Brain, 3; Diarrhœa Acute, 13; Diarrhœa Chronic, 12; Dentition, 7; Debility, 22; Dropsy, 2; Diphtheria, 4; Drowned, 1; Dysentery Acute, 9; Dysentery Chronic, 21; Delirium Tremens, 2; Dyspepsia, 4; Epilepsy, 1; Febris, 7; Enteritis, 2; Eclampsia, 4; Gastritis, 10; Heart Disease, 2; Hemorrhage of Lungs, 2; Hydrocephalus, 8; Hepatitis, 8; Intermittent Fever, 2; Inflammation of Bowels, 3; Inflammation of Brain, 3; Marasmus, 13; Meningitis, 4; Pneumonia, 15; Phthisis, 9; Peritonitis, 2; Paralysis, 2; Pertussis, 3; Poison, 2; Premature Birth, 8; Remittent Fever, 4; Rheumatism, 1; Still Born, 30; Syphilis, 1; Suicide, 2; Spasms, 4; Summer Complaint, 8; Scrofula, 2; Tuberculosis, 1; Tetanus, 1; Trismus, 9; Typhoid Fever, 30; Tabes, 2; Unknown, 6; Whooping Cough, 4; Wound, 1. Total, 493.

United States, 333; Ireland, 58; Germany, 78; Bohemia, 4; Italy, 3; England, 2; Scotland, 3; Switzerland, 6; Prussia, 2; Austria, 1; Canada, 1; Bavaria, 2. Total, 493.

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REMARKS.

a. For reading, writing, etc., there is no need of glasses.

b. Need of glasses for distant vision.

c. The best glasses are those which, by improving sight, do not diminish the size of objects, or very slightly so.

d. In marked myopia, correct at first one third of the extent of accommodation, then another third, etc. (GIRAUD-TEULON.)

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